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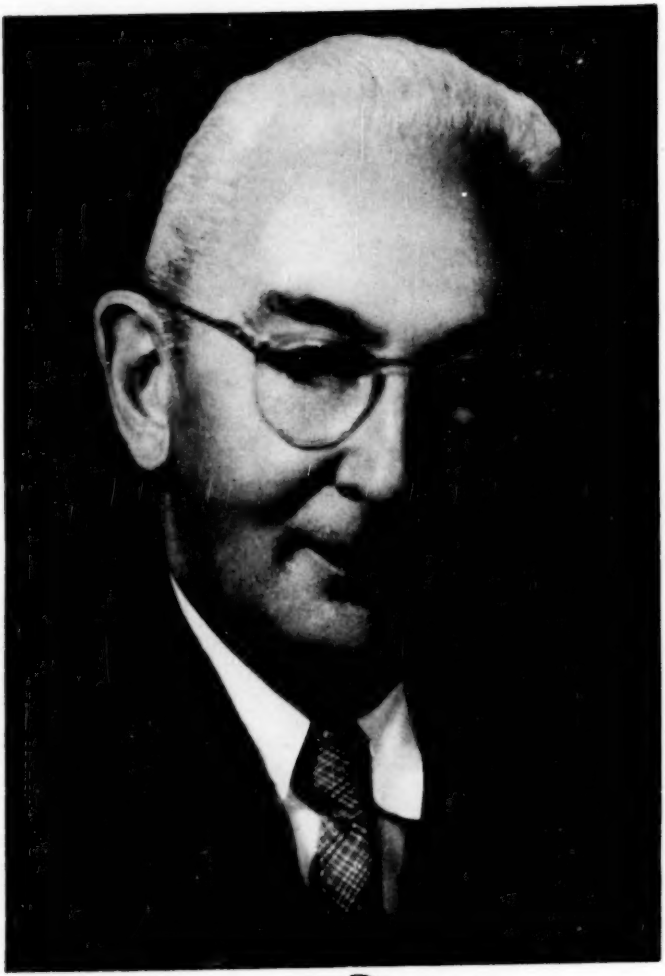

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THE PSYCHOLOGICAL REVIEW

STEVENSON SMITH: 1883–1950

Stevenson Smith, Professor of Psychology at the University of Washington, was born in Philadelphia on April 29, 1883. His early life was spent in Philadelphia and in California, with many summers in Europe. He attended the University of Pennsylvania with his brother, Henry Bradford Smith—later Professor of Philosophy at that University—receiving his B.S. in 1904, and remaining there for his first year of graduate study. After a summer at Heidelberg, he studied for a year under Frederick Tilney at the College of Physicians and Surgeons, Columbia University. Following this, he returned to the University of Pennsylvania where he received his Ph.D. in 1909. It is interesting to note that Smith went to Tilney with the request that he be permitted to devote himself solely to the study of neurology. He was not seeking a degree in medicine, but only that solid foundation in neurology which he felt was essential for the proper understanding of problems in psychology. This excellent foundation was reflected in his work in child and clinical psychology. His first teaching assignment was at Hampden Sydney College from 1909 to 1911. On the recommendation of E. L. Thorndike, he was appointed Assistant Professor of Orthogenics at the University of Washington in 1911, where, under the Gatsert Foundation of Child Welfare, he started the first child clinic west of the Mississippi. In 1915 a separate department of psychology was established, and Smith served as the Chairman of the Department of Psychology and Director of the Institute of Child Welfare until 1948. The last two years of his life were devoted to teaching and research. His last publication appeared in September, 1950.

His interest in child psychology is most clearly seen in the records of the Institute of Child Welfare. From the time of its establishment until 1940, over 13,000 children were given psychological guidance in Smith's clinic. But in addition to this Smith advanced the cause of child welfare in the State of Washington through committee work, drafting of preliminary laws and urging the establishment of psychological clinics in the school systems. Along with his efforts in behalf of child guidance and the preparation of analytical reports and recommendations on each of the 13,000 children that passed through his clinic, Smith published several articles of a practical and theoretical nature on the problems of child guidance. The last particular publication in this field was an essay, in his exceptionally charming and readable style, on "The Psychological Origin and Treatment of Enuresis."

Simultaneously with his work in clinical psychology, Smith maintained a very extensive interest in general and theoretical psychology. His writings range from his first paper "The Limits of Edu-

cability in Paramoecium" through the problems of handedness, stammering, memory, motor skills, specific conditioners, exhibitionism, psychological tests, statistical diagnosis of monozygotic and dizygotic twins, children's opinions, intelligence and physical characteristics of different racial groups, to stereoscopic depth perception. Behind all of his writings, and certainly fundamental to all of his psychological thinking, was his interest in the theory of learning. Many psychologists will remember the tremendous impact on American psychology of Smith and Guthrie's "General Psychology in Terms of Behavior." This was the first textbook to take real advantage of the contributions of behaviorism without denying and abandoning many of the central problems of psychology. It was in this volume, too, that Smith's interest and contribution to the theory of learning in terms of association first began to appear. And, of course, several papers specific to learning theory followed. One is always struck with the clarity and neatness with which Smith defined a problem, planned an experimental attack on it, carried out the experimental work and published a paper which was clear, concise and to the point. It will be a loss to psychology that Smith was unable to write his book on child psychology. It would have been a book of practical treatment of the problems of child behavior, but it would have carried with it a very systematic consideration of the nature of these problems. Undoubtedly, the therapy and the systematic treatment would have been in terms of learning theory.

R. B. Loucks' resolution passed by the University of Washington Senate on the occasion of Smith's death includes, "Thousands of people within the state will remember Stevenson Smith because of their contacts with him in the clinic. Many of his colleagues on the faculty will remember him because of his strength of character and his penetrating wit that was so effective a weapon against pretentiousness. His numerous friends within professional groups can never forget his indomitable spirit, his personal integrity and his unwillingness to tolerate anything that was second-rate, however appealing it might appear to the uninformed and uncritical. The members of his own department will always be influenced by the memory of his uncompromising standard for academic investigation."

GEORGE PLANT HORTON
University of Washington

AN EVALUATION OF SOME CURRENT CRITICISMS OF GESTALT PSYCHOLOGICAL WORK ON PERCEPTION

BY ABRAHAM S. LUCHINS

McGill University

To ascertain the nature of the perceptual process has become a key problem in the fields of social, personality, and clinical psychology. Some students of these fields complain that past research on perception has developed "laws" and "principles" which may hold fairly well in darkroom and laboratory situations, but which do not yield an adequate understanding of perception in the clinic or in everyday life situations (*cf.* 1, p. 100). They are therefore seeking to "reexplore much of this ancient field of perception whose laws for too long have been taken for granted" (1, p. 108); they are therefore endeavoring to take a "new look" at perception.

In the writings of some of these "New Look"[1] adherents—to borrow Krech's appellative (19, p. 86)—one finds the need for and the virtues of their proposed approaches highlighted by criticism directed against past research in perception. No small portion of this criticism has been leveled against the work on perception done by exponents of Gestalt theory. For example, Bruner and Postman claim that the great majority of experiments conducted by the major Gestalt experimentalists have "coolly snubbed" the perceiver (2, p. 77), have sidestepped the key issues of the organism's creativity (2, p. 77), and have underemphasized the activity of the organism (2, p. 79). Cantril claims that in their views on perception Gestaltists have tended to neglect the dimension of time, including the experiences of the past and the expectancies of the future (5, p. 145), and notes that ". . . when the role of expectancies is added to the role of past experiences in determining perception, the 'immediate present' becomes much less significant than it is considered to be by gestalt psychologists" (5, p. 145). Brunswik claims that Gestaltists have not dealt adequately with the distal object relationships involved in perception (4, p. 57). Krech claims that most of the Gestalt psychologists (excluding Lewin) have neglected the "motivational aspects" of behavior (19, p. 77). These and numerous other sins of omission and commission have been attributed to Gestaltists. The present report will attempt to evaluate some of the critical remarks aimed at Gestalt psychological work on perception, as well as some of the attempts of New Look investigations to compensate for the alleged shortcomings of past research.

[1] For brevity of exposition we shall employ this appellative to denote such work on perception as that reported in (1, 2, 3, 4, 5, 13, 21, 28, 33, 34), not thereby intending to imply a new "school" of psychology, in any sense of the word.

Some Preliminary Remarks

Interpretation "From Above"

At the roots of the New Look movement in perception, we have noted, there lies a dissatisfaction with the failure of the "laws" and "principles" of perception, developed in laboratory studies, to yield a full understanding of perception in everyday life situations. Interestingly enough, figuring prominently in the development of Gestalt theory was a similar discontent with the discrepancy between the work of the

laboratory and the "actual spectacle of human life" (17, p. 143), or, more generally, between the promulgations of science and the world of everyday events (39). It seemed to Wertheimer that the difficulty stemmed from a fundamental assumption of the science of his day—namely, that the only scientific method was the method of traditional analysis. He characterized this method as working "from below upward," *i.e.*, as attempting to divide complexes into their component elements, to isolate the elements and derive their laws, and then to combine them for comprehension of the phenomenon (6, p. 4). As the fundamental formula (*Grundproblem*) of Gestalt theory, Wertheimer postulated the following:

> "There are wholes, the behavior of which is not determined by that of their individual elements, but where the part-processes are themselves determined by the intrinsic nature of the whole. It is the hope of Gestalt theory to determine the nature of such wholes" (6, p. 2).

Such wholes (or *Gestalten*) are characterized by determination "from above," *i.e.*, parts are determined by the intrinsic conditions of their wholes and are to be understood as parts relative to such wholes. Gestalt theory holds that such whole-phenomena cannot be adequately comprehended as the mere sum of parts, and cannot be adequately studied by studying their parts in isolation, since outside of their particular situation the parts in question would not remain what they were within it (17, p. 143). Interpretation "from above," from whole-properties downward to subsidiary wholes and parts, was advocated by Wertheimer as a method of dealing with whole-phenomena. The inadequacy of the psychology of his day to explain everyday occurrences he attributed to the fact that such events are often characterized by determination "from above" while psychologists were attempting interpretations "from below" (39). Thus he claimed that when one asked psychology what it had to say about a living experience, one was given "an assortment of elements, sensations, images, feelings, acts of will and laws governing these elements—and was told, 'Take your choice, reconstruct from them the experience you had'" (6, p. 4).

It should be pointed out that a procedure "from above" does not imply neglect of the existence of parts of wholes nor neglect of the need for *analysis*. Rather, it is held that (a) the study of whole-properties and whole-conditions may lead to the discovery of genuine parts of wholes and to the real significance of the parts (37, p. 55; 18, p. 168) and (b) that the investigator has to be on the lookout for modifications which his method of analysis may produce in the part under consideration or elsewhere in the whole to which the part belongs (37, p. 53). In this connection note Köhler's statements that one of the main tasks of Gestalt psychology is that of indicating the genuine rather than any fictitious parts of wholes, and that analysis in terms of genuine parts is "a perfectly legitimate and necessary procedure" in Gestalt psychology (18, pp. 168–169).

Gestalt Psychology and "Pure Perception"

It may be asked: If Gestalt theory arose out of a discontent with the discrepancy between everyday life and the psychology of the laboratory, why is it that it turned to the laboratory study of perception, so that at present Gestalt psychology is often associated with "pure perception" rather than with what some New Look adherents describe as "social perception"?

To begin with, we note that in his lectures Wertheimer sometimes charac-

terized the opinion that Gestalt theory is concerned only or mainly with perception as being nothing more than "a fairy tale" (*cf.* also 17, p. 143). As a matter of fact, the first report in which he attempted to demonstrate the applicability of Gestalt methods was not, as is popularly believed, his experiment on stroboscopic movement (36) but rather his paper on numerical concepts in primitive peoples (35) [2]—and fittingly, his last contribution also dealt with thought processes (43). Not only in perception and human thought, but in every branch of psychology, as well as in other disciplines, he found examples in which the principle of determination "from above" operated; hence he was fully aware that the principle of interpretation "from above" was applicable to diverse phenomena (*cf.* 17, 39). That he turned to investigations of perception can be accounted for on the grounds that these investigations served as convenient vehicles for demonstrating in a precise manner the methods and concepts of Gestalt theory. In particular, while he recognized the importance of studying everyday acts of perception in all their richness and complexity, he first turned to the more modest task, as he sometimes phrased it in his lectures, of studying the perception of dots and lines (36, 38), both for reasons which will be noted in a subsequent section and because he was not unhopeful (a) that the concepts developed in such studies would have general applicability, and (b) that the investigations would lead to the development of scientific tools and procedures which would permit further research with more meaningful material of everyday life. That Gestalt methods and principles have been applied to complex phenomena of everyday life is evident from a perusal of the literature (*cf.* 14–18, 40–44).

Moreover, the criticism that Gestalt psychologists have tended to focus on *pure perception* rather than on *social perception* raises the problem of what is meant by the italicized phrases. In spite of the prevalence of the latter phrase in current reports, there is no general agreement as to its intent. For example, Bruner and Postman begin with the proposition that "*all* perception is to a lesser or greater degree social perception" (2, pp. 71–72). Cantril labels as a *social perception* one involving a "two-way relationship between potential purposes," *i.e.*, where the stimulus "has a potentiality of affecting our purposes and being affected by us" (5, p. 149). As examples of stimuli for social perception he cites a hornet, a man, a political ideology, since they may do something to or for us and we may do something to or for them. A pencil and lightning, he states, may be considered as non-social perceptions because the latter can do something to us while we can do nothing to it, and because we can do something to the pencil while it cannot itself do anything to us. Brunswik, on the other hand, employs the term *social perception* not as it refers "to the influence of social factors on perception at large, but rather to the attainment of what may be called covert-distal traits of personality and

[2] In this paper he attempted to demonstrate that meaningful counting is not necessarily an and-summative process, that structural group-apprehension may underlie the conception of quantity, that plurality need not be a quantity of identical items but may involve an articulated whole. But he was far from satisfied with the presentation, and sought for a demonstration which would permit clearer formulations of his ideas, one which would permit clear-cut decisions. While on a train trip he conceived of utilizing stroboscopic movement as a basis for a crucial investigation. He got off at Frankfurt and conducted the preliminary experiments on the "phi phenomenon" in his hotel room, using a child's plaything, which showed "moving pictures" when it was revolved, as the basis of his apparatus, and having Doctors Köhler and Koffka serve as observers.

intelligence in other people by way of, say, intuitive snap judgments" (4, p. 61). It would appear that considerable clarification is in order concerning the meaning or meanings of the terms "perception," "pure perception," and "social perception," and the distinctions or differences, if any, among them.

CONCERNING THE ASPECTS OF PERCEPTION

Some of the New Look investigators claim to reveal or at least to deal with various "aspects" of perception. Thus McGinnies tells us that during the past decade "a number of experimental investigations have progressively revealed the so-called 'dynamic,' or motivational aspects of perceptual behavior" (28, p. 244). Klein and Schlesinger are concentrating on the "defensive aspects" of perception (13, p. 36). Bruner and Postman (3) and Brunswik (4) are concerned with the "veridical" aspects or the "functional" aspects of perception. Gestalt psychological research has been chided for neglect of such "aspects" (*cf.* 19, p. 81).

Perhaps only a matter of terminology is involved here, different writers employing different terms to describe the activity of perception but not thereby intending to imply diversified aspects. This would not seem to be entirely the case. Consider, for example, that Klein and Schlesinger divide perception into "defensive aspects . . . direct wish-gratification . . . creative and synthetic aspects" (13, p. 36). They state that they are concentrating on the first and using it as a guide in constructing the model of the person as a perceiver. (In order to obtain a complete model of the perceiver, are we then to construct models of him based on each of the four alleged aspects of perception? And how are we to fuse these four models in order to obtain a model of the entire perceiver?)

The aspects which a particular psychologist imputes to perception seem to be dependent on his particular theoretical orientation. The question arises as to whether the study of one or even all of these theoretically-derived aspects will necessarily yield an understanding of the perceptual process. Do we know enough about perception to be able to so divide and compartmentalize it, or may such division violate the very nature of the phenomena under investigation?

Now let us consider whether or not exponents of Gestalt theory have neglected these "aspects" in their research on perception.

Dynamic Aspects

That Gestalt psychologists should be accused of having neglected so-called dynamic aspects of perception or of having been non-dynamic in their approach is somewhat ironic in view of the fact that the basis of Gestalt theory and research is the dynamic or field view as opposed to a "machine theory" (18, p. 354). "Dynamics plays so slight a role in contemporary theory," wrote Köhler in the concluding paragraph in his chapter contrasting dynamics and machine theory (first published in 1929), "that the terms which have been used in the preceding paragraphs may sound a bit mysterious to many a psychologist" (18, p. 134). Indeed, Gestalt-oriented investigations, particularly Wertheimer's study of stroboscopic movements (36), have been credited with introducing dynamic concepts into the study of psychological phenomena (*cf.* 45, p. 124).

Then why are Gestalt psychologists now regarded by some as having neglected the *dynamic* aspects of perception? An answer may be found in the confusion concerning the meanings of such terms as dynamics, dynamic determinants, and dynamic aspects. Ge-

stalt psychological literature generally employs the dynamic concept as it refers to stresses and strains in a field of force (*cf.* 14, 15, 16, 18). *Dynamic determinants* of a system have been defined by Köhler as the forces and other factors inherent in the processes of the system, in contrast to *topographical factors* of a system which subject its processes to restricting conditions (18, p. 107). The dynamic view, he holds, is one which acknowledges direct determination rather than "connections" in the sense of machine theory (18, p. 352).

In the writings of many of the New Look adherents, such terms as *dynamics, dynamic determinants,* and *dynamic aspects* often appear from their context to refer to covert or unconscious tendencies, to psychoanalytic mechanisms, to a dialectical process or struggle between forces in the individual, or to a struggle between the individual's "inner strivings" and the "demands of reality" (*cf.* 13, p. 36). In short, it has become fashionable to offer or imply a psychoanalytic interpretation of dynamics. Indeed, in some circles, to be psychoanalytically inclined means to be "dynamic," and vice versa—so that any other orientation or interpretation is automatically non-dynamic. Specifically, the dynamics of perception as dealt with by Gestaltists may be regarded as not genuinely dynamic because of the lack of a psychoanalytic orientation. Perhaps the New Look adherents who describe their work as dealing with the dynamic aspects of perception do so because the theory of personality which underlies their investigations stems from psychoanalysis. It is questionable, however, that the "dynamics" of the perceiver's personality, as conceived of by a certain theory of personality, need necessarily contain a description of or an explanation of the dynamics of an act of perception.

It seems to us that careful consideration of the meaning associated with the concept of dynamics (including the significance to psychology of the manner in which the concept is employed in physics), and minimal use of the concept and related terminology without explicit definition, would seem to be very much in order (*cf.* our criticism along these lines in 26).

Motivational Aspects

Analysis of the contention that Gestalt psychological research has neglected the motivational aspects of perception raises the problem of what is meant by such aspects, which in turn calls for examination of some of the assumptions underlying current theories of motivation. A popular assumption is that the motives which underlie behavior necessarily stem from or reside in the self, *i.e.,* that motivation is self-generated. A related assumption is that all behavior is motivated in the sense that it serves some purpose for the organism. Explicit or implicit acceptance of these assumptions is evident in several of the current reports on perception (*cf.* 1, 5, 13, 28). For example, Klein and Schlesinger speak of the person as the source and root of motives whose influences are sought (13, p. 32), and assume that a person is always in a state of motivation, *i.e.,* "never without purpose" (13, p. 36).

Consider the following critique formulated by MacLeod of the assumption that motivation is necessarily self-generated.

> "If we try to envisage all motivation as self-generated, we are doing violence to some of the more obvious facts of observation. The vectors may emerge from the object just as validly as from the self. The self may be remodelled to meet the demands of the object just as truly as the object may be reshaped to fit the needs of the self" (30, p. 65).

Thus, we may be "guided by an ideal, repelled by a threat, or lured by the beautiful maiden" (30, p. 65). In short, the so-called tertiary value-qualities of objects may serve to motivate our behavior.

With reference to the second assumption, the question arises: Must behavior be motivated, in the sense that it is "useful" to or serves the behaver, or may there be non-motivated behavior? In this connection note Hebb's critical analysis of prevailing concepts of motivation (9, pp. 171–207). Note also that in a forthcoming paper Maslow plans to deal with "the important task of challenging and calling into question the generally accepted belief that all behavior is motivated," that is, that it is *useful* to the behaver (31, p. 261).

Gestalt theorists do not see convincing grounds for accepting the theses that motivation is concerned solely with the wishes and needs of the organism, and that the source of the motivation is necessarily the self. If motivation is so regarded, then it is true that Gestalt psychology proper has not greatly concerned itself with motivation *in this sense*. Apropos of this we quote from Köhler's discussion of Wertheimer's work. After noting that in motivation there are innumerable examples of determination "from above," Köhler states:

> "It may seem surprising that for years Wertheimer never mentioned motivation when he discussed determination 'from above.' The reason is that motivation is generally believed to concern only the subjective interests of individual selves, and Wertheimer was forever suspicious of mere selves. He despised that little voice which keeps whispering: Where do I get my share? Nothing would have appeared to him more unfortunate than the assumption that things make sense only with reference to the subjective wishes of an I. He admitted no necessary connection between the general concept of determination 'from above' and the particular demands of a self. Thus he turned away from problems of motivation. Later, he thought, when fundamentals were clear, subjective motivation would be more readily recognized as what it is: not the essence of the new principle (determination 'from above') but a special instance to which it also applies" (17, p. 144).

The belief that Gestaltists have neglected motivational aspects may stem from the assumption that they have been concerned mainly with developing principles of "perception" and "cognition." In studying behavior Gestalt psychologists do not tend to subdivide it into perceptual, cognitive, and motivational aspects, in order to find the concepts and principles applicable to each. Wertheimer held that the dynamics of motivation are fundamentally the same as those of perception and thought, and that the same basic concepts and principles may apply to each (*cf.* 17, p. 43). Similar conceptions are to be found in the writings of the other major Gestalt theorists (14, 18). In a recent report on perceptual constancy MacLeod concluded: "If the hypotheses outlined here are correct, we shall demonstrate not merely that perception and motivation are governed by parallel laws, but that the laws of perception and the laws of motivation are the same" (30, p. 65). *Cf.* also Krech's account of the difficulties and "pseudo problems" involved in regarding "cognition" and "emotion" as two basically separate functions or processes, and then seeking to "interrelate" them (19, p. 82).

Defensive Aspects

Among the investigators claiming to be concerned with defensive activity in perception are Bruner and Postman (3), McGinnies (28), and Klein and Schlesinger (13). The latter speak of the motive of defense, which they regard as closely related to the psychoanalytic concept of defense, as the prime or the

over-all motive of perception (13, pp. 36, 46). They hold that "stimulation from the welter of the outside world . . . with free entrance, would traumatize and overwhelm us" (13, p. 37), but that the motive of defense helps the perceiver to maintain "equilibrium between inner strivings and the demands of reality" (13, p. 36).

Such concepts are not popularly subscribed to by Gestalt psychologists but, aside from a predilection for psychoanalytic concepts, what is the basis for assuming that the prime motive in perception is defense, that perception is a struggle between the person and the "outside world," between wish and reality-press? What is the basis for assuming, as do Klein and Schlesinger, that the diversity of the perceptual system should reflect personal defensive solutions? Why defensive solutions? Bruner and Postman's concern with defensive activity in perception stems from their interest in determining how the person comes to represent the geographical field in the behavioral field, how the organism by perceiving comes to adapt to the external, distal stimulus (3, p. 29). But is the process underlying the representation of the geographical field in the behavioral field adequately characterized as a process of "adaptation" to the distal stimulus, and is this "adaptation" actually characterized by defensive activity?

Parenthetically, we note that both Bruner and Postman (3) and Brunswik (4) criticize Gestalt psychology for not dealing adequately with distal object relationships. Yet they themselves deal with the process underlying such relationships only by attempting to ascertain what perception does for the perceiver, what purpose it serves for him—thus attempting to explain a *process* in terms of an *outcome* (*cf.* MacLeod's warning against such "explanations"; 30, p. 60). Moreover, while Gestalt psychologists do not claim to have explained adequately the relationship between the geographical and behavioral fields, or between the geographical and the brain fields, they certainly have been interested in such relationships. In this connection, note, for example, Köhler's chapter on the nature of the physical world (15, Chapter V). It seems to us that Gestalt psychologists have not been *exclusively* concerned with the isomorphism of brain fields and the structure of experience, nor have they accepted as "the end-all of psychology" the analysis of the behavioral field and its isomorphism with the brain field—accusations recently promulgated by Bruner and Postman (3, p. 29).

Functional Aspects

Related to the defensive aspects of perception are the so-called functional aspects. Gestalt psychologists have been described as having neglected the functional while stressing the formal or structural aspects, of having been formalistic or structuralistic rather than functionalistic in their approach to perception. Thus Bruner and Postman classify Gestalt psychologists as *Formalists* rather than *Functionalists* (3, p. 14). Egon Brunswik concurs with this "identification of formalism and Gestalt psychology" (4, p. 56), refers to the one-sided formalism of Gestalt psychology proper (4, p. 56n), and describes it as among the least functionalistic of the "six distinct types of perception psychology" with which he is concerned (4, p. 64). Several of the New Look investigators state or imply that they are oriented toward functionalism (*cf.* 3, 4, 5). Indeed, the *raison d'être* sometimes advanced for functionalism is that it constitutes a protest against "nineteenth-century structuralism" (4, p. 64) or against Gestalt psychology's "formalism" (3, p. 29).

This classification of Gestalt theory may be related to the distinction which is sometimes drawn between the Functional school of psychology (*cf.* 10, Chapter 6) and the Structural school (*cf.* 45, Chapter 2). Categorization of Gestalt psychology in the latter "school" places it among strange bedfellows, for Gestalt theory is opposed to the piecemeal, atomistic mental chemistry, to the stimulus-sensation formula of some of the so-called Structural psychologies (*cf.* 45). In this connection note Koffka's remark that Gestalt theory "may be compared with that of American Functional Psychology since in both cases the principle of a fixed relationship between stimulus and sensation (Structural Psychology) has been discarded in favour of a biological point of view: the organism in its environment" (6, p. 377). That Gestalt theory has been concerned with structures is not denied. But its conception of the nature of structures and its approach to their study are overlooked if one simply classifies Gestaltists as structuralists or formalists. Now, we do not intend the above to imply that Gestalt psychology should be classified in the Functional school, whether it be "nineteenth-century functionalism" or "current functionalism." But then, is there any particular virtue in classifying Gestalt theory or any other approach to psychology?

It has been implied that Gestalt theory has concerned itself mainly with the structure of the nervous system and the structure of experience (*cf.* 3, p. 29). But Gestalt psychologists do not conceive of thick boundaries necessarily separating structure from function, particularly as it refers to the living organism. Thus Köhler notes, "In the organism the dependence of structure on various functions goes in fact so far that here the distinction between structure and function may sometimes almost appear as arbitrary" (15, p. 292).

Contrary to some current criticisms, functional concepts hold a respected position in Gestalt theory. Gestalt psychology attempts to study constellations and processes as functional wholes (18, p. 178), studies functional relations (18, p. 248), and applies functional concepts to sensory organization (18, p. 178). Indeed, the physiological theory advanced by Gestalt psychologists has been referred to as a *functional* theory and its development has been described as an attempt to deal with "functional problems . . . in functional terms" (18, p. 125).

Then why is it maintained by some that Gestalt psychology neglects functional concepts in perception? Basal to the apparent dilemma are the meanings associated with such terms as *functional, structural,* and their derivatives. We have already had occasion (25) to refer to the confusion resulting from various and undeclared meanings being attached to such terms as *structure* and *structured.* The situation with regard to the concept of function is no clearer. Consider, for example, that three meanings of the concept of function have been listed by Woodworth: (a) use or value; (b) operation or process, *i.e.,* internal function and not external use; (c) mathematical function (45, pp. 28–29). Other conceptions of the term "function" are to be found in the literature (*cf.* 15, p. 292). If one is restricted to the three meanings listed by Woodworth, then it can be said that Gestalt psychologists have tended to employ the concept of function as if its referent were (b) or (c) above, whereas many of the New Look psychologists tend to employ it as if its referent were (a) above; *i.e.,* as if it referred to the use or value or purpose for the perceiver. Thus, a functional approach of the kind advocated by Cantril conceives

of perception "as part and parcel of purposive activity" (5, p. 145). The kind of functionalism advocated by Brunswik and his associates is described as *utilitarian* (4, p. 61). Bruner and Postman's type of functionalism inquires into "what perceiving does for the organism" (3, p. 29). While Bruner and Postman describe their approach as extreme functionalism, Brunswik does not regard their "type of functionalism . . . as utilitarian in the same cognitive sense of the word as are the two (just discussed) branches of distal veridical functionalism" (4, p. 61). Among the various kinds of functionalism which Brunswik (4) refers to are probabilistic functionalism (p. 57), environment-oriented objective functionalism which he describes as necessarily probabilistic functionalism (p. 60), American functionalism (p. 61), motivational functionalism (p. 62), the "so-called functionalism of the act-psychologists" (p. 64), and the "genuine functionalism" of thing-constancy and social perception (p. 64).

It may be then that the sizzling controversy between functionalism and structuralism fizzles down to the meaning or meanings you choose to attribute to either of the terms. Seriously, it seems to us that the categorization of Gestalt psychologists as structuralists or formalists as opposed to functionalists, unaccompanied by careful analysis of the meaning and bases of the classification; the sharp distinction which tends to be drawn in some circles between function and structure or between function and form (compare Murphy's criticism of this dichotomy; 32, p. 51); and the classification, which we shall discuss in subsequent sections, of perceptual determinants and perceptual principles into the two categories of structural and functional, all testify to a predilection for creating sharp, seemingly neat, but often misleading dichotomies which at best tend to becloud the issues to which they are applied.

CONCERNING THE DETERMINANTS AND PRINCIPLES OF PERCEPTION

The factors or determinants or characteristics of perception are sometimes divided into two major categories: structural (autochthonous, Gestalt) on the one hand, and functional (motivational, behavioral) on the other. The latter have been defined by Krech and Crutchfield as those factors which "derive primarily from the needs, moods, past experience and memory of the individual" (20, p. 82), and the former as those factors "deriving solely from the nature of the physical stimuli and the neural effects they evoke in the nervous system of the individual" (20, p. 81). More simply, "functional" factors are sometimes characterized as forces "from within" the self, and "autochthonous" factors as "forces from without."

Gestalt psychologists have been said to have emphasized the autochthonous or structural factors while neglecting the functional factors. In the words of Bruner and Postman: "As long as possible and as hard as possible, the major Gestalt experimentalists in the field of perception have been concerned with autochthonous factors in perception: similarity, contrast, likeness of fate, *Prägnanz*" (2, p. 77). In contrast, some of the current studies are concerned with demonstrating the existence and importance of the functional (behavioral, motivational) determinants of perception. Some even imply a "victory" of one or more of these determinants over one or more structural or autochthonous determinants. Thus Bruner and Goodman offer a possible interpretation of their experimental results as representing "one more autochthonous factor outweighed by the behavioral determinants discussed in the course of this paper" (1, p. 108). Such interpre-

tations are not unusual. As Krech and Crutchfield state in a discussion of the structural and functional factors involved in perception: "Experimental and theoretical literature in perception psychology is replete with discussions as to the relative importance of these two sets of factors" (20, p. 81). To promulgate as the goal of theoretical discussions or experimental investigations the determination of the relative importance of the "two sets" of factors seems to us to constitute a rather fruitless endeavor until it has been established that there are two such distinct sets whose importance can be compared. In the following paragraphs we shall attempt to establish that division of the determinants and principles of perception into "two sets" is based largely on arbitrary and misleading distinctions.

Terminology of the Dichotomy

Consider firstly the terminology of the dichotomy. Why are the terms "structural," "autochthonous," or "Gestalt" applied to one set of determinants, and the terms "functional," "motivational," etc. applied to the other set? What are the meanings of the terms in this context?

In the literature of Gestalt psychology the terms "structural" and "autochthonous" generally refer to *Gestalten*, to wholes and whole-processes. Thus in a discussion of *Gestalten*, Wertheimer states that "we are considering structures with their concrete structural principles" (6, p. 15). Fuller understanding of the meanings of these terms would then seem to call for an understanding of the concept of Gestalt. This concept, Köhler (18) has pointed out, has the connotation of shape or form as an attribute of things, as well as the meaning of a concrete entity *per se*, which may have a shape as one of its characteristics (pp. 177–178). He asserts that Gestalt in the meaning of form or shape is no longer at the center of the Gestalt psychologist's attention (p. 179). According to the "most general functional definition" of the concept of Gestalt, it may be applicable to "the processes of learning, of recall, of striving, of emotional attitude, of thinking, acting and so forth" (p. 179).

The concepts of Gestalt as well as Gestalt or structural principles are applicable and have been applied not only to forces "from without" but also to forces "from within" the self, not only to physical stimuli but also to the person and his needs, moods, past experience, and memory (*cf.* 14–18, 40–44). Hence the designation of structural or Gestalt might be applied to factors in either of the "two sets" of perceptual determinants. Therefore, the description of one "set" of determinants, and not the other, as "structural" (or "Gestalt" or "autochthonous") appears to be highly arbitrary.

Application of the term "functional" to (A) the factors which stem primarily from the individual's needs, moods, etc., and *not* to (B) the factors which stem from the physical stimuli and their corresponding neural effects, raises the question of what this term is intended to imply. If it is to be understood as referring to use or value to the perceiver (*cf.* 45, pp. 28–29), then is the distinction intended to imply that factors B have no use or value to the perceiver? If the term "functional" is intended to refer to internal process or function (*cf.* 45, pp. 28–29), then is the dichotomy intended to imply that factors B are not functional in this sense? Or is still another meaning of the term "functional" involved?

Again, is characterization of factors A and *not* factors B as motivational intended to imply that the latter may not serve to motivate behavior? Because factors A are seen as deriving from the perceiver, self, or ego, does it follow

that they are more "motivational" than factors which derive from the physical stimuli? We have already had occasion to criticize the conception that motivation must be self-generated.

In summary, it would seem that some clarification is required concerning the terminology of the dichotomous classification of the determinants of perception. It seems to us that the dichotomy rests largely on semantic distinctions which tend to break down upon close examination.

Distinction in Terms of Neural Effects

The cited definition of structural determinants of perception refers to the physical stimuli and the *neural* effects they evoke in the nervous system of the individual while the cited definition of functional determinants does *not* refer to neural effects. Is this intended to imply that the perceiver's needs, moods, past experience and memory do *not* evoke neural effects in his nervous system? Or is it intended to imply that the term "functional" applies to the needs, moods, etc. *per se,* but not to their corresponding neural effects? Or is it perhaps intended to imply that the neural effects evoked by needs, moods, etc. are different physiologically or otherwise than those evoked by extra-organismic physical stimuli? According to Gestalt theory, the self and its needs, moods, memory, etc. evoke neural effects, possess cortical correlates, give rise to cortical processes, which are not necessarily different in kind from those corresponding to extra-organismic stimuli (*cf.* 14, Chapters IX–XIV; 18, Chapters VII–X).

It may not be out of place to mention that Gestalt psychologists' attempts to understand the cortical correlates of perception are sometimes interpreted as implying that they have not dealt with perception as dependent upon the "whole organism." Thus, Murphy applies a hierarchical scheme to "types of research" and states:

> "Beginning at the bottom, we would have studies of the functions of receptors and sensory neurones. A little higher up, perception would appear in relation to the central nervous system, as brilliantly exemplified in such recent work as that of Köhler on the direct currents in the brain involved in figural aftereffects. At a higher level we should have perceptual responses dependent upon the whole organism as an organism. Here we should have the interactions of central and autonomic nervous systems in the simpler autistic responses . . ." (32, p. 53).

The important matter is not that such work as that of Köhler's is placed in a lower level than the subsequent "type of research" (Murphy points out that this categorization of types of research does not intend invidious distinctions between them) but that cortical processes are regarded as not dependent on the whole organism. In contrast, note Köhler's statement that local processes manifest a clearly defined organization "relative to the needs and conditions of the organism as a whole" (6, p. 55). Note also Wertheimer's statement that the cells of an organism are parts of the whole and excitations occurring in them are thus to be viewed as "part-processes functionally related to whole-processes of the entire organism" (6, p. 15).

It may be retorted that in their actual studies of perception Gestaltists have not dealt adequately with the conditions of the entire organism. Perhaps. But do the studies of the "simpler autistic responses," *e.g.,* Schafer and Murphy's investigations of "autistic responses" (34), deal adequately with such conditions? Theoretically, at least, Gestalt theory regards cortical processes as related to processes of the entire organism.

Distinction in Terms of Direction of Operation

In the reports of some New Look adherents, "autochthonous" determinants of perception (or forces "from without") are referred to as constraining or limiting forces, invoked when it is necessary to explain why the perceiver's wishes and needs do not completely determine the percept, and in general treated as if they were sentinels guarding reality against the onslaught of "motivational" determinants (*cf.* our formulations in 26, p. 83). For example, Klein and Schlesinger speak of the person as one who must maintain equilibrium "between inner strivings and the demands of reality," as one who must "buffer these turbulences from within (his needs, impulses, wishes) against limitations from without" (13, p. 36). Are forces "from within" necessarily *turbulences*, and forces "from without" necessarily *limitations* to one's needs, impulses or wishes? One may be seeing here the influence of the pleasure-reality conflict, with forces "from within" treated as if they represented the pleasure principle and forces "from without" the reality principle.

This distinction between the "two sets" of perceptual determinants might be characterized as a difference in the direction of operation, *i.e.*, so-called structural determinants are considered to be working to preserve reality in sensory experience and so-called functional factors are considered to be working for perceptual distortions or errors. Such conceptions appear to underlie the choice of some recent titles, *e.g.*, "The relation of the intensity of a need to the amount of perceptual distortion" (21). They probably underlie Klein and Schlesinger's admission, apropos of the influence of personal values on perception, that "Value was frankly expected to be disruptive, to have an error producing effect" (13, p. 34). This distinction in terms of direction of operation is well reflected in the formulation found in Bruner and Goodman's report on judgments of sizes of various coins: "It would seem that, for all values below a quarter, equivocality has the effect of making judgments conform more to actual size, aiding, in other words, the operation of autochthonous determinants. For values over a quarter, equivocality favors behavioral [functional, motivational] factors, making apparent size diverge still more from actual size" (1, p. 107).

Gestalt theory does not regard forces stemming primarily from the perceiver and his needs, moods, etc. as necessarily leading to distortion in sensory experience of the nature of the physical environment, but recognizes that under certain conditions such forces may even help to make one better aware of the nature of the physical environment. Moreover—and this seems to us to be worthy of underscoring in view of current misconceptions—it does not regard forces stemming primarily from the physical stimuli and their corresponding neural effects (so-called structural, autochthonous, or Gestalt factors) as working to reproduce faithfully in sensory experience the properties of the physical environment. Köhler pointed out that there are "many exceptions to the correspondence of sensory organization and physical facts" (18, p. 156), and that the correspondence between the Gestalt properties of nervous processes and the Gestalt properties of the objects they represent is "by no means universal" (6, p. 393). For example, one may experience motion, as in the phi phenomenon, where motion did not take place in the geographical environment (36), or experience a unit when there is no stimulus continuum but only discrete dots (38). These "distortions" of the physical environment as well as

numerous other examples, *e.g.*, so-called perceptual illusions, can be accounted for in terms of Gestalt factors and Gestalt processes (*cf.* 14, 18). In short Gestalt theory holds that the direction of operation of a force is dependent on the total situation in which it is involved and cannot be ascertained, once and for all, by classifying or categorizing the force into a given set.

The weakness, then, of this distinction between the "two sets" of perceptual determinants, lies in the underlying erroneous assumption that the direction of operation of a force is a function solely or primarily of its derivation, source, or origin (*i.e.*, deriving from the perceiver or the physical stimuli, "from within" or "from without").

The Two Sets of Principles

We have noted that not only the determinants but the very principles of perceptual organization are frequently grouped into two categories. Is it necessarily meaningful that there should be two sets of principles governing perceptual organization? Perhaps the fact that quantitative laws of how the so-called functional factors operate in perception are *lacking* (20, p. 83) is not due to a want of zeal on the part of investigators searching for the "functional principles" of perception, but rather to the fact that there is not one set of principles governing the operation of the "functional" factors and another set governing the operation of the "structural" factors. Directly applicable to this dichotomous classification of the principles of perceptual organization are most of the arguments which we raised against the corresponding classification of perceptual determinants.

Interrelatedness of Two Sets

It should be pointed out that many investigators who deal with the so-called functional factors of perception or who are seeking for the so-called functional principles, recognize that the "two sets" of factors (and principles) are interrelated, "that whatever perception is being observed is a function of both sets of factors" (20, p. 83). However, we see little point in seeking to study the interrelatedness between the "two sets" until it is established that there *are* two sets to be interrelated.

To paraphrase Krech (19, p. 82): There are New Look investigators who recognize that these two sets of factors or principles are not independent, but instead of taking the bold step of exploring the possibility that they are not dealing with two sets, they continue to think in terms of this dichotomy and seek the answer in studying the interaction or the interrelatedness of these two. We may say, along with Krech (19) and Hochberg and Gleitman (11), that for all their talk about unity of behavior or unity of perception, these interactionists or interrelaters are still "separatists."

Possible Bases for the Dichotomies

Why is it that so much time, effort, and printers' ink have been devoted to the discussion of the relative importance of these "two sets" of perceptual determinants and principles? In part the answer seems to us to lie in interpretations and misinterpretations of Gestaltists' views on perception.

Thus to Gestalt theory is sometimes attributed the view that perceptual organization is determined solely or mainly by the structural or autochthonous or Gestalt properties of *stimulus objects*; hence current investigators feel the need repeatedly to demonstrate the existence and importance of other determinants of perceptual organization, particularly those stemming from the perceiver—the so-called functional determinants.

For example, after referring to the so-called dynamic or motivational aspects of perceptual behavior, supposedly revealed during the past decade, McGinnies exclaims, "No longer do we view perception as organized solely in terms of the structural characteristics of stimulus objects . . ." (28, p. 244). We are tempted to ask: When was perception so viewed? Certainly Gestalt psychology did not and does not view perception as organized *solely* in terms of the structural characteristics of stimulus objects, whether these be the distant (physical) stimulus objects or the proximal ones (*cf.* 14, p. 80). Sensory organization, Köhler emphasizes, is an achievement of the nervous system, with the formation of specific units occurring in neural functions (18, p. 160). He mentions that the purely physical stimulus-manifold may not "exhibit any whole-coherence worthy of practical consideration" (6, p. 390n). For example: "When someone plays on the piano, organized processes take place within him, but the physical wave-progression brought about by his activity constitutes no organized whole" (6, p. 390n). Köhler criticizes the interpretation that according to Gestalt psychology, *Gestalten*, in the sense of segregated entities, exist outside the organism and simply extend or project themselves into the nervous system. Believing that this misapprehension may stem from misinterpretation of the thesis of psychophysical isomorphism, he points out that this thesis refers "to the similarity between sensory experience and accompanying physiological processes" and "cannot simply be applied to the relationship between such processes and the physical environment" (18, p. 160n).

Related to the above is Bruner and Postman's critique that Gestalt research on perception has concerned itself with the "properties of the stimulus" and underemphasized "the activity of the organism" (2, p. 78). But if sensory organization is an achievement of the nervous system and if the formation of specific units occurs in neural functions (18, p. 160), then is not perception a function of activities of the organism? These same investigators hold that "the great bulk of the perceptual research published by the exponents of Gestalt interpretations of perception sidestepped the key issues of the organism's creativity" (2, p. 77). This might be answered by stating that Gestalt theory regards the organism as the "creator" of the percept, and the creation is none the less genuine because it is not regarded as essentially arbitrary. As a matter of fact, the creativity attributed to the organism in Gestalt theory has been singled out for mention by some New Look adherents. For example, while Brunswik criticizes Gestalt psychology for being insufficiently functionalistic, he vindicates in this respect "the creative aspects of Gestalt processes" (4, p. 64).

Another plausible basis for the current interest in the relative importance of the "two sets" of perceptual determinants may be the interpretation that Gestaltists have underemphasized the importance in perceptual organization of factors which stem from the perceiver, self, or ego. Now it is true that Gestaltists have stated that determination "from above" does not mean that the determination of the roles of parts in larger structures need necessarily stem from a self. Thus Wertheimer maintained that vectors which determine parts need not at all issue from the self, but may issue from the larger structure to which the parts belong (*cf.* 17, p. 144). But to maintain that the vectors which determine parts within larger structures *need* not issue from the self is not to deny that they *may*, particularly since the self may belong to the larger structure to which the parts in question belong. In short, while Gestalt theorists would maintain that

forces originating in the self are not the only forces determining perceptual organization, and that the operation of forces stemming from the surrounding field has to be taken into account, they would hold, along with Koffka, that forces "starting in the Ego can take effect in the field and co-determine its articulation" (14, p. 197). In a subsequent section we shall consider some of the perceptual determinants, commonly regarded as stemming from the perceiver, whose importance Gestaltists have been accused of neglecting or understimating.

SOME FACTORS IN PERCEPTION ALLEGEDLY NEGLECTED BY GESTALTISTS

Past Experience

We shall deal at some length with the claim that Gestaltists have underestimated the importance in perception of the factor of past experience (*cf.* 2, p. 77; 5, p. 145). We ask the reader to bear in mind the popularity, both in the past and at present, of "past experience" as an explanation of perception, as in empiristic accounts of behavior. To a considerable extent the direction of research by Gestalt psychologists was shaped by the need for coping with empiristic objections. That in his famous paper on laws of organization of perceptual forms (38) Wertheimer selected as illustrations the grouping of separate dots and lines has been accounted for by Köhler on the grounds that "when such patterns rather than continuous objects are used demonstrations are less open to objections in terms of previous knowledge" (18, p. 144). That they conducted experiment after experiment to prove their positive statements and to discredit empiristic theory has been explained by Koffka in these words: "We could consider our experimental proof of the effectiveness of our organizing factors as amply sufficient, had we not to contend with the vested interests of an old theory (empiricism) which claims to explain all our facts as well as we do, but without the assumption of all these different organizing forces" (14, p. 155).

It is important to note that in the aforementioned paper (38) Wertheimer did include the factors of set and of past experience or habit among the factors which determine the organization of perceptual forms. Bruner and Postman claim that such factors were assigned a distinctly secondary place in Wertheimer's laws of perceptual grouping (3, p. 14). In answer, we point to Wertheimer's statement that there "can be no doubt" that some of our apprehensions are determined by means of past experience or habit (6, p. 86). Actually the importance of the various factors mentioned by Wertheimer was regarded as dependent upon the concrete situation in which they were involved: "By appropriate variations [any] factor may be weakened or strengthened" (6, p. 77). That he did not experimentally attempt to strengthen the factor of past experience is understandable in light of his attempt to study the principles governing "spontaneous grouping," grouping which could not be explained solely on the basis of such factors as past learning. We cannot agree with Bruner and Postman that the exponents of Gestalt theory "felt it to be some form of treachery to work intensively with the last of Wertheimer's *Gestaltfaktoren:* past experience" (2, p. 77).[3] There

[3] The present writer has been very much interested in studying the operation of certain kinds of past experiences on the perception and solution of certain problem situations (22), on the perception and judgment of lengths of line segments (23), and on the perception and description of certain complex drawings (24). He was encouraged to engage in such "treacherous" activities by none other than the late Max Wertheimer.

was no question of treachery involved; the problem facing Wertheimer and his associates was not to defend the factor of past experience but to demonstrate that there were other factors determining perceptual organization, and that such organization need not be due to previous learning or experience or to attitudes or to any other "secondary" process imposed on the parts.

Gestalt psychologists are not opposed to the conception that past experience may influence present behavior. What they do attack is (1) the explanation that all organization is based on past experience, and (2) the conception that past experience, in the sense of sheer repetition of an item of experience, always exerts an automatic influence on perception. This seems to us to be clearly stated or implied in the following formulations from Wertheimer and Köhler, respectively, as cited in Ellis (6).

"There can be no doubt that some of our apprehensions are determined in this way (by means of past experience or habit). Often arbitrary material can be arranged in arbitrary form and, after a sufficient drill, made habitual. The difficulty is, however, that many people are inclined to attribute to this principle the fundamental structure of *all* apprehension" (6, p. 86).

"It would be extremely unfortunate if the problem (of visual articulation) were thrust aside at this point as being after all only another case of the influence of past experience. No one doubts that past experience is an important factor in *some* cases, but the attempt to explain all perception in such terms is absolutely sure to fail, for it is easy to demonstrate instances where perception is not at all influenced by past experience" (6, p. 58).

It was the general explanation of perception in terms of "past experience" (where this phrase refers to sheer repetition of an item of experience) which the studies of Gottschaldt (7, 8) attacked. But he did not imply that past experience is never an important factor or that it plays no role in perception. Yet his work is sometimes so misconstrued. Thus Hilgard claims that Gottschaldt's experiment is "repeatedly cited by gestalt psychologists in order to disprove the role of experience" (10, p. 185). If so, then the "gestalt psychologists" concerned are drawing a generalization which Gottschaldt did not make.

Bruner and Postman refer "to the experiments of Gottschaldt which had as their aim to demonstrate that since under certain conditions experience was not an effective factor in organizing perception, *ergo*, experience was not an important factor in perception" (2, p. 77). This conclusion did not appear in Gottschaldt's writings. He asked whether past experience is able to exert modifying effects upon any and all figures, and concluded that there are *some* cases where "past experience" (in the sense of bare repetition) is practically without effect (7, p. 277). From this he inferred that important changes should be made in "experience theory," *e.g.*, that the theory should renounce its claim to universal validity and indicate instead its own range of application (7, p. 278). He does not argue that past experience is universally an unimportant factor.

Gestalt psychologists have also attacked the implicit or explicit assumption that the operation of experience is necessarily in the nature of an and-summative process marked by contextual indifference, externality, and adventitiousness. "Not *that* it makes use of experience causes our objection to empiricism, but *how* it makes use of it," commented Koffka (14, p. 639n). Apropos of depth perception Koffka wrote that he would be very wary in denying that experience has any influence on depth, but that "before we know what experience means, the introduction

of experience has no explanatory value and only when we understand experience as a process of organization itself will it help us in our present problem" (14, p. 161). Wertheimer held that experience need not be regarded "as a bare sequence of events each essentially alien to all the rest" (6, p. 16). Köhler has noted that Gestalt principles are applicable to the temporal sequences and processes involved in experience (18, p. 62).

According to Gestalt theory, to say that a particular phenomenon is due to "past experience" does not solve a problem but raises new ones. What are the principles governing the occurrence of "experience"? What are the principles governing its operation as a determinant of behavior? What roles do experiences play in structuring and restructuring one's perceptions? Can the operation of past experience be understood in terms of Gestalt or field principles or is it essentially a piecewise, arbitrary matter? (Such problems unfortunately have not been a matter of concern to some of the New Look adherents who tend to rest content with the assertion that past experience of one kind or another influenced the subjects' perceptions.) In short, Gestalt theory is not satisfied with the mere catchword "past experience" but sees the need for experimental inquiry into the principles governing the formation and operation of experience, as well as experimental inquiry into the principles governing the phenomenon supposedly explained by the expression "past experience."

We think that it is in order to refer here to the fairly common conception that because Gestaltists reject empiristic explanations, they must automatically prefer nativistic explanations, that the only alternative to empiricism (or environmentalism) is nativism. This conception seems to us to be well expressed in the following quotation from Hilgard.

> "Gestalt psychologists . . . have a *preference for nativism*, in the sense that they account for the interaction of organism and environment largely in terms of the way in which the organism is made. In perception, for example, it is argued that seen motion, third dimension, and other such features attributed by associationists to learning are instead functions of contemporary arrangements, *independent of prior experience*" (10, pp. 10–11, italics ours).

As a reference for this quotation Hilgard cites Koffka's text on Gestalt psychology (14). Yet in this very text there is ample evidence to contradict the gist of the quotation. Thus Koffka rejects both the nativists' and empiricists' views on space perception (p. 160), and offers instead the hypothesis that "three-dimensional shapes are matters of organization in the same way as two-dimensional ones, depending on the same kinds of laws" (p. 161). In discussing fixation he indicates that "a rejection of the empiristic explanation does not improve the case for the nativistic one" (p. 313), but offers instead an explanation differing from both nativistic and empiristic accounts. He notes that the "traditional nativistic theory was unable to cope with accommodation" (p. 316). Eye movements, he states, "find their explanation, *not in facts of anatomical structure*, but in facts of systemic equilibria produced by processes of organization," thereby refuting a nativistic interpretation (p. 317, italics ours).

Köhler (18) states that all order which is found in mental events tends to be explained in terms of either inherited machine arrangements or secondarily acquired constraints, representative of nativistic and empiristic explanations, respectively (pp. 112–113). He criticizes the notion that the only alternative to a nativistic account is the empiristic one, offering as "a further alternative" the hypothesis that function may be orderly when neither inherited

nor acquired arrangements in the nervous system are responsible for the order (p. 113). In discussing constancy of brightness and size, he claims that "neither the empiristic nor the nativistic assumptions can be right" (p. 117), and notes again that they are not the only alternatives. He discusses the nativistic and empiristic interpretations of the fact that experiences of the various sense modalities are localized in a common space, and offers a Gestalt-oriented interpretation as "a third explanation" (pp. 214–215).

These are not isolated instances. Nowhere in the writings of the leading Gestalt theorists, as far as we know, is there a stated or implied preference for nativism. That Hilgard (10) and others infer such a preference is evidence of the prevalence of the assumption that if one does not lean toward empiristic interpretations one must have a predilection for nativistic ones—an assumption heartily disavowed by Gestalt theorists. Gestalt theory, Wertheimer repeatedly pointed out in his lectures, is not in line with either the nativistic or empiristic orientations, but represents a new and different approach.

Needs and Values

It may not be digressing too far afield to mention that Gestaltists have been intensely concerned with the problems of needs and values. As a matter of fact, Köhler has devoted an entire book (15) to such problems. However, they do not regard needs and values as necessarily attached by the perceiver, in an arbitrary manner, to objects in his environment, nor do they regard the source of the needs and values as necessarily the self. In contrast, some of the New Look adherents who claim to be concerned with the role of needs and values in perception (*e.g.*, 1, 21, 33) deal with these as if they were subjectively and arbitrarily related to objects in the perceiver's environment. Perhaps the particular needs and values which they have selected for experimental investigation are not at all determined by requiredness and fit (*cf.* 15). But will an investigation of them necessarily yield an understanding of the operation in perception of needs and values which are more intrinsically related to the situations in which they are involved? This seems to us to be a matter worthy of some serious consideration.

The picture which we painted, based on the expressed views of Gestaltists concerning the role of past experience in perception, would hold with little modification for the role of personal values or needs. They do not deny that such factors may influence our perceptions but insist that they cannot account for all perceptions. Moreover Gestalt theorists would not be content with the bare statement that personal needs or values influence perception, but would be likely to inquire: What are the principles underlying the formation of such needs and values, and what are the principles underlying their operation as determinants of one's percepts? Are the processes best studied by a procedure "from above" or "from below"? In short, the same kind of dissatisfaction which we noted concerning the catch-all of "past experience" as an explanation of behavior would hold with reference to the popular contention that needs and values, often regarded as operating to serve "autistic" drives, influence perceptions.

Attempts have been made by some of the New Look investigators to indicate that personal needs and values can operate as organizing factors in perception (*cf.* 1). Unfortunately it is not made clear in what sense the term *organization* is employed or in what sense personal values and needs are *organizing factors*. Indeed the terms "organization" and

"organizing" are being employed so glibly and loosely in current psychological discussions, apparently meaning many things to many people, that they are rapidly becoming empty phrases. It may therefore not be out of place to institute a procedure which Wertheimer enforced in his seminars—namely, the term "organized" and related terminology were not acceptable, in and of themselves, as explanations or descriptions of a phenomenon; the speaker had to indicate explicitly in what manner the phenomenon was organized, and just what its organizational features were.

Attitudes

As a last example, consider some of the views expressed by Gestaltists with reference to the roles of attitudes in perception. We shall include herein the factor of expectation which some psychologists regard as underemphasized in Gestalt psychological work on perception (*cf.* 5, p. 145).

In his writings Köhler repeatedly refers to the roles played by attitudes in maintaining and altering sensory organization (*cf.* 18, pp. 114, 117, 120, 169, 263, 300, 328, 345, 347). Gottschaldt comments on the significance of the subject's attitude in helping to determine whether he would see the "hidden figures" in the more complex figures (7, p. 277). In discussing Gottschaldt's experiments, Koffka states that the attitude of the observer at the moment of the exposure had a very direct influence, but that there were definite limits to such an influence, *e.g.*, that an expectation for the *a* figure (the "hidden figure") would not produce an *a* organization when the *a* figure was by structural principles completely lost in the more complex patterns (14, pp. 397–398). Koffka notes that "the field organization under certain circumstances is dependent upon attitudes, *i.e.*, forces which have their origin not in the surrounding field at all, but in the Ego of the observer" (14, pp. 148–149). In discussing the effects of attitudes, he concludes that attitudes "have a definite influence on the field organization which may overcome contrary forces of internal organization" (14, p. 398).

In short, Gestalt psychologists have recognized the roles which attitudes may play in perception. What they have attacked is the conception that attitudes in general or special attitudes, such as expectation or attention, can account for all perceptual organization (*cf.* Köhler's arguments as presented in 6, pp. 383–384). Moreover, they have raised the same kinds of questions concerning the nature, formation, and operation of attitudes (*cf.* 6, p. 384; 18, p. 300; which we have already discussed with reference to past experience, needs, and values.

SOME EVALUATIVE REMARKS CONCERNING NEW LOOK INVESTIGATIONS

Some of the previously discussed criticisms of Gestalt psychological work on perception are predicated on the assumption that certain aspects of investigation, or certain results or outcomes, are inadequate, in and of themselves, to explain all perception. Now, to date, to the best of our knowledge, no Gestalt psychologist has claimed to have revealed the complete and final truth about perception. Moreover, as Wertheimer pointed out, Gestalt theory is concerned with actual research; it is not only an outcome but a device, not only a theory about results but a means toward further discoveries (6, p. 3). Are the current criticisms aimed at these devices, these means toward further discoveries? Are they aimed at the *methods* of working which characterize Gestalt-oriented research? Do they disagree with the *Grundproblem* of Gestalt theory—that there are wholes which cannot be understood merely as the and-

summation of their parts? Do they indicate that interpretation "from above" is an inadequate process for learning more about perception? In short, are the current criticisms intended to imply that a Gestalt or configurational or field orientation must be sidetracked if subsequent research on perception is to be more fruitful?

Answers to these questions appear to be in the negative. Almost without exception those who currently criticize Gestalt psychological work in perception readily embrace the conception that perception is organized, that it involves whole-processes, that one must study perception as a whole, the perceiver as a whole. There would probably be little disagreement with Gardner Murphy's belief that for "a long time to come . . . research will certainly have to be field-oriented" (32, p. 54), or with Bruner and Postman's assertion that the system of description of the perceptual world "must be a configurational system" (2, p. 112). Indeed the spirit of the major charges against Gestalt psychologists might be summarized by the contention that in their work on perception they have been "inadequate Gestaltists" (19, p. 77)—that they have dealt with artificial laboratory situations rather than with everyday situations, and that they have failed to encompass "everything" in the total field (2, p. 77), with the ego, self, or perceiver being the hapless victim of the oversight and the consequences being results and principles inadequate to explain everyday perceptions.

Indeed considerable ado is being made over the discovery that there are not only *Gestalten* but Gestalters, that there are not only perceptions but perceivers, and that the perceiver (person, self, ego) is part of the total field. Mere statement of these facts is not very revolutionary; Gestalt psychologists have been well aware of them. Thus Koffka noted "that our task of investigating the surrounding field alone is somewhat artificial and that we shall understand its organization completely only when we study the total field which includes the Ego within its environment" (14, p. 149). As early as 1923 Wertheimer made the following remarks, translated and condensed by Ellis (6).

> "Our next point is that my field comprises also my Ego. There is not from the begining an Ego over-against others, but the genesis of an Ego offers one of the most fascinating problems, the solution of which seems to lie in Gestalt principles. However, once constituted, the Ego is a functional part of the total field. Proceeding as before we may therefore ask: What happens to the Ego as a part of the field? Is the resulting behavior the piecewise sort of thing associationism, experience theory, and the like, would have us believe?" (6, p. 6).

The crucial issue centers on the development and utilization of methods with which to study the total field. Are New Look investigators employing such methods and applying them to the study of situations in the clinic and the marketplace? Are they studying *the perceiver* and the part he plays in the total field? Are they encompassing "everything" in the field? In short, are they overcoming the alleged shortcomings of past research on perception conducted by exponents of Gestalt theory?

1. The experimental setups utilized in many of the New Look investigations are by and large a far cry from situations of everyday life or of the clinic. Indeed, several of the investigators use the same kinds of devices and situations (*e.g.*, threshold phenomena) which have been dubbed as artificial, laboratory situations.

Moreover one of the favorite tools in current research in perception is the tachistoscope. But it is doubtful that the tachistoscopic presentation of, say,

emotionally-charged words (as in 28), represents the prototype of perceptual situations of the marketplace and the clinic, or that tachistoscopic presentations—say, rapid, successive presentation of the same geographical stimulus (as in 33)—preserve the unity and continuity characteristic, on the phenomenal level, of much of our everyday perceptions. (*Cf.* the criticisms along these lines contained in 26.)

2. It seems to us that in their actual research, particularly in their interpretation of results, some of the New Look adherents tend to neglect the possible influence of (a) the experimenter and other personnel present at the time of experimentation, (b) the particular wording of the instructions to the subject, (c) the interpretation which the subject gives to the instructions and to his task, and (d) the particular conditions which the particular subject faces. In short, it seems to us that adequate account is not taken of specific field conditions.

3. The current trend toward a person-orientation seems sometimes to overshadow a field-orientation. Consider, for example, Klein and Schlesinger's plea for a person-oriented approach in which the locus of study becomes "not the *situation*, but the *person* and how he is organized to cope with his values and needs" (13, p. 35). Let us even assume that the phrase "the situation" does not refer to the total field or to the total experimental situation, but only to the experimental setup or stimuli which are presented to the subject by the experimenter. Nonetheless the problem remains: Must the locus be the person *or* the situation? Is it not feasible to attempt to study the total field, including the perceiver, the "situation," the experimenter, etc., allowing each to reveal its proper role and function? Must one focus on the perceiver and develop "laws of perceivers" (13, p. 32), and then focus on the "situation" and develop laws of situations, and then interrelate them? Would this not be reminiscent of traditional analysis which sought to isolate the components, discover the laws of the components, and combine them for comprehension of the phenomenon? And is such an approach advisable for the study of perception?

4. It is interesting to observe that the perceiver is sometimes regarded as synonymous with his needs, values, past experiences, etc. (*cf.* 2, p. 77), so that studies which purport to deal with the perceiver set as their goal the study of the influence on perceptions of personal needs, values, past experiences, etc. Does the conglomeration of needs, values, expectations, habits, etc., make up the entire perceiver? And is dealing with one or more of these factors equivalent to studying the perceiver?

Instead of studying *the perceiver* and his particular values, needs, interests, many New Look investigations are concerned with group trends to which the individual subject is perforce subordinated, with hypothetical mechanisms of perceptual functioning, with abstractions related to personality, and with generalized needs, values, interests, the nature and intensity of which are often arbitrarily determined by the experimenter. In this context consider the study by Bruner and Goodman entitled "Value and need as organizing factors in perception" (1). The investigators make what they describe as the "reasonable assumption" that poor children have a greater subjective need for money than rich ones (1, p. 106). But is it entirely reasonable to assume that the need for money is inversely related to the amount of money possessed by one's family? The values that a particular child placed on money and the particular circumstances which might have influenced the child's need for it at the time of the experiment were not

taken into account. It seems to us to be a far cry from determining a subject's subjective need for money to assuming arbitrarily that any child in the "poor group" had more of this "need" than any child in the "rich group."

It is interesting to note that even Klein and Schlesinger (13), who criticize other recent research for neglecting the particular perceiver, nonetheless end up by advocating a study of *normalizing mechanisms* (p. 36), of relationships between two sets of behavior in terms of which they can *group* people (p. 42), and of *perceptual types* (p. 45). One might well ask them, paraphrasing the question which forms the title of their report, Where is the perceiver in this theory?

5. Yet we do not doubt that a hypothetical perceiver or ego has become the idol of much of the current research on perception. At times it seems that, because of service to this idol, investigators tend to lose sight of their goals. An interesting case in point seems to be furnished by the following illustration. In 1947 Bruner and Goodman stated:

> "The discrepancy between the dark room and the market place we have in the past found convenient to dismiss by invoking various *dei ex machina:* Attention, Apperception, *Unbewusster Schluss, Einstellung,* Preparatory Set, etc. Like the vengeful and unannounced step-brother from Australia in the poorer murder mysteries, they turn up at the crucial juncture to do the dirty work. Though such constructs are useful, perception itself must remain the primary focus. To shift attention away from it by invoking poorly understood intervening variables does little service" (1, p. 100).

Yet in the interim Bruner and his associates have found it convenient to invoke such poorly understood intervening variables as *Unbewusster Schluss* and Preparatory Set (2), and such poorly understood intervening mechanisms (intervening "between motivation and perception," 3, p. 18) as sensitization, defense, value resonance, vigilance, and primitivation (28, p. 244). Note the assumption that playing host to the functioning of these mechanisms "is the 'ego,' in whose service, presumably, the various perceptual adjustments operate" (28, p. 244).

In line with the cited quotation from Bruner and Goodman, we may say that to invoke such poorly understood intervening variables or mechanisms "does little service" for the study of perception, even if they are regarded as serving some hypothetical ego. The fact that service to the ego is posited seems to obliterate the fact that perception no longer is "the primary focus."

6. Some of the New Look investigations appear to be attempting to compensate for an alleged "underemphasis" on perception in past research by a corresponding "overemphasis." Now, if one pays at least lip service to the conception of a field-orientation, as do most of the New Look adherents, then such stresses on the part of the investigator may constitute shortcomings to their research possibly as grievous as those they are attempting to overcome. Thus if it is a fault to neglect the perceiver, does it not constitute another fault to stress the perceiver to the extent that other factors and total field conditions are neglected? (In this connection we call attention again to the so-called person-oriented approach in which the locus of study is the person and not the situation, 13, p. 35.) If it is a fault to stress the contribution of the stimulus constellation more than that of the organism, is it not another fault to stress "the contribution of the organism more than that of the stimulus configuration" (4, pp. 61–62)? (The latter, incidentally, is a characterization of Bruner and Postman's research made by Brunswik, a fellow "functionalist.") If underem-

phasis on the so-called functional determinants of perception is deplorable, is it not also deplorable to overemphasize them? (Ketchum has noted a tendency in current reports towards overemphasis on functional determination which he deems "dangerous, both theoretically and practically," 12, p. 221.)

In short, some of the New Look investigations tend to deal with alleged deficiencies of A, B, C, . . . in past research by giving us an overabundance of A, B, C, ... Is this necessarily the answer? Has it been established that the antidote for an underemphasis is a corresponding overemphasis? Must the emphasis or stress stem from the investigator; or would it be best if the *concrete phenomenon under investigation* were allowed to reveal what should be stressed, if it were allowed to reveal its own areas of major importance?

CONCLUDING REMARKS

It seems to us that in New Look investigations there is a tendency to begin with conceptual schemes, theoretical orientations, and theories concerning personality and perception, and too little working *from* actual acts of perception *to* theoretical explanations. Thus assumptions stemming from personality theory, particularly psychoanalysis, tend to be uncritically applied to perception. Similarly New Look investigators tend to divide perception into dichotomous and rather arbitrary classifications on the basis of their theoretical biases, and then to turn to experimental investigations in order to demonstrate the existence or importance of the theoretically-derived aspects, determinants, and principles.

In this working from theoretical biases we have quite the reverse of the configurational approach which involves, to use MacLeod's words, "first an attempt at an unbiased description of phenomena, then systematic experimentation designed to reveal the essential determinants of these phenomena, and finally revision of existing theory in the light of the new principles discovered" (29, pp. 196–197).

MacLeod has made this configurational approach the basis of what he describes as a "phenomenological approach" to social psychology (29). A phenomenological emphasis, he notes, does not restrict the psychologist to the description of phenomena but requires him "to look first at the world of things-as-they-are in its entirety before deciding which aspects of this world are to be considered important for theory, and at every stage in his investigations to keep checking back to phenomena to make sure that they have not been distorted by the very process of investigation" (29, p. 197).

At the present time, when controversies concerning theoretically-derived aspects, determinants, and principles of perception seem to be cluttering up psychological discussions of perception and confusing the issues involved, the most productive approach may be just the aforementioned "phenomenological" or configurational approach. While a completely unbiased description of phenomena may not be attainable, the observer can attempt explicitly to formulate his biases or assumptions and to determine their possible influence on his observations. It may be advisable for various observers with different biases to report their observations, so that some understanding may be gained of the roles which various assumptions play in determining one's observations of the phenomena. The psychologist observing an act of perception should not glibly accept assumptions, even such Gestalt-flavored ones as "all perception is organized," or such analytically-flavored ones as "perceptual responses serve the ego," but should at-

tempt to formulate his assumptions, suspend them if possible, or attempt to ascertain their possible influence on his observations, his analysis and interpretation of data.

For the psychologist to apply such an approach to actual acts of perception in the laboratory, the clinic, and the social field does not require him to accept the philosophical implications of phenomenology or Gestalt theory or positivism. It does require him to be a good scientist, and to permit the phenomenon at hand to speak for itself, to suggest its essential determinants, to suggest the kinds of questions which should be put to it, and the kinds of experimental variations which should be conducted in order that crucial answers be obtained, in order that one penetrate to the essential nature of the phenomenon. In Wertheimer's words, it means that "we must be guided . . . by the concrete nature of the things themselves which we are studying" (6, p. 3).

SUMMARY

A "New Look" movement has developed in perception, ostensibly guided by the goal of understanding perception not only as it takes place in the confines of the darkroom and the psychology laboratory but as it occurs in the clinic and in everyday life situations. Unfortunately this goal seems to be forgotten while investigators busy themselves with criticizing past research in perception, and with ascribing to themselves while denying to others the "virtues" of being functionalistic, dynamic, or motivational in their approach. We have been particularly concerned with some of the verbal brickbats thrown at the research on perception conducted by exponents of Gestalt theory, and with some of the interpretations and misinterpretations of Gestalt views.

We criticized the current tendency to divide perception into various "aspects," *e.g.*, dynamic, motivational, veridical or functional aspects. We also criticized the division of perceptual determinants and principles into two major categories: functional (motivational, behavioral), on the one hand, and structural (autochthonous, Gestalt), on the other. It was indicated that such divisions and categorizations tend to be based largely on arbitrary and misleading distinctions which involve semantic confusion. To set as the goal of psychological discussions or investigations the problem of the "relative importance" of any of these theoretically derived aspects, determinants, or principles, or even the problem of their interrelatedness, seems to us to constitute a rather pointless endeavor until it has been established that there are actually various "sets" to be compared or interrelated.

We attempted to establish that Gestaltists have not neglected "functional concepts," that they have not been "non-dynamic" in their approach, that they have not neglected various "aspects" of perception or various "sets" of perceptual determinants and principles. We indicated that Gestalt psychologists do not draw a generalization concerning the "relative importance" of any perceptual determinant, nor do they view its importance as ascertainable from the "set" or category in which it is placed. In particular we noted that, contrary to prevalent current beliefs, Gestaltists do not regard so-called autochthonous (structural, Gestalt) factors as necessarily working faithfully to reproduce in sensory experience the nature of the physical environment.

Gestalt psychologists have been accused of underestimating the importance in perception of such factors as past experience, needs, values, attitudes, expectations. We indicated that they recognize the role which such factors may play in perception. However, they are

not satisfied with the cliché, "past experience," but see the need for experimental inquiry into the principles governing the formation and operation of experience, as well as experimental inquiry into the principles governing the phenomenon supposedly explained by the expression "past experience." Similar formulations were drawn with reference to Gestaltists' views on other factors.

We criticized the conception that because Gestalt psychologists reject empiristic explanations, they must automatically lean toward nativism; we attempted to establish that Gestalt theory is not in line with either the nativistic or empiricistic orientation, but represents a different approach.

The gist of some current criticisms might be summarized by the statement that in their research on perception Gestalt psychologists have been "inadequate Gestaltists," that they have failed to include "everything" in the total field. We have attempted to show that New Look researchers are, by and large, not more adequate Gestaltists, that they have not dealt with "everything" in the total field, that they have tended not to deal with the perceiver as part of the total field, or with the "entire perceiver," or even, with a particular perceiver and his particular needs, values, experiences, etc.; rather, they tend to deal with group trends and with abstractions relating to personality and perception. We noted also a tendency among New Look adherents to ignore, in their actual research, situations of the marketplace and clinic, but to adhere to setups which may be dubbed as artificial laboratory situations.

It was pointed out that some New Look adherents tend to deal with an alleged "underemphasis" on the part of the Gestaltists by introducing a corresponding "overemphasis." We wondered whether the scientific antidote for an "underemphasis" is an "overemphasis," whether the resultant of the two stresses will necessarily yield an understanding of the phenomenon. We asked why the stress must stem from the experimenter, why the concrete phenomenon under investigation could not be permitted to reveal what should be stressed.

We noted that several of the New Look adherents tend to begin with theoretical biases and conceptual schemes rather than with actual acts of perception. As a more fruitful method of approach we advocate the configurational or phenomenological approach which starts with careful observation in which the observer tries to formulate and if possible to suspend his biases or assumptions concerning the phenomena under consideration; which goes on to systematic experimentation involving systematic variation of the conditions under which the phenomena are studied; and which, sometimes, ends with theoretical explanations of the phenomena.

It is our hope that the present report will clarify some of the controversies involved in current criticisms of Gestalt psychological work on perception and in current attempts to compensate for its alleged shortcomings. It is our hope that this report will serve to draw attention to the confused state of affairs in discussions of perception, and to the fact that promulgated dichotomies are not as clear-cut as they may appear at first glance, but that they are based largely on semantic distinctions which tend to break down on close examination. Finally we dare to hope that some of the time and effort currently devoted to attacking other theories or defending conceptual schemes concerning perception and perceivers will be turned to the careful study of actual perceivers and actual acts of perception in the clinic, the social field, and the laboratory.

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[4] This text contains condensations, in English, of various reports originally published in German, including references (7, 8, 35, 37, 38, 39) of the present bibliography. While in referring to citations from this text (*e.g.*, 6, p. 3) we have generally credited the cited quotation to the original author (*e.g.*, Wertheimer), it should be kept in mind that the rendition into English is not generally a direct translation of the original German.

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THE EFFECT OF PERSONAL VALUES ON PERCEPTION: AN EXPERIMENTAL CRITIQUE [1]

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The Menninger Foundation

Although we take it for granted by now that value and need *can* affect perception, it is not always easy to specify under what conditions this influence will occur. Bruner and Postman (2) tried to demonstrate the influence of value and need by showing that the mere presence of a value symbol, such as a dollar sign or a swastika on a disc, caused its size to be overestimated. This "accentuation" suggested to them that what is important to a person looms larger in his perception. It is doubtful, however, as Carter and Schooler have shown (3), whether this simple formula covers all instances in which value is a factor in the situation.

Bruner and Postman's study appears to have skirted or overlooked several sources of variation which might have contributed importantly to their results. Certainly more needs to be known about the interrelationships of valu*er*, value, and the experimental conditions. To our mind, it is equally important to look closely at the size-estimation *method* which gave birth to their formula, for it seems to us to rest on dubious assumptions about the relation of personality and perception which must inevitably color generalizations from it and limit its usefulness as a tool. Therefore, we believed it worth while to repeat the Bruner-Postman experiment in such a way as to highlight additional variables and to focus upon the usefulness of the method itself as a model for investigations in this area.

Following the main outlines of the Bruner and Postman model, our experiment examined the influence of four additional variables upon the task of size-estimation: (1) the intensity of the value, (2) the difficulty of the task itself, (3) the figural properties of value-stimuli, (4) the gross presence or absence of any figure, value or neutral. Since the effect of a value on perception might depend on its strength, one would want to know about the relative intensity of the (induced) value among *S*s. Again, a perceptual task might be performed under such simple and stable conditions as to be impervious to influence by the strongest of value stimulation while in more complex and "difficult" perceptual situations a value might have a pronounced effect. The third variable rests upon a suspicion that systematic errors of size-estimation might be due to the formal aspects rather than the value-significance of the figures. These are by no means the only relevant variables which demand study. The most important one of all, the way in which individual *S*s are organized to cope with values, a variable conveyed in such concepts as ego-structure and defensive structure, we cannot consider at all in this experiment, though we will discuss it briefly.

PROCEDURE

Apparatus: The experimental apparatus was a black box, 18" long, 18"

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wide, and 9" deep. Its front panel held two circular ground-glass panes, 4" in diameter and 6" between centers. Behind the right pane (the variable stimulus field) was a 12-leaf iris diaphragm which either *S* or *E* could adjust. Behind the left pane (standard stimulus-field) was a slide holder which displayed different standard discs and figures. Each field was illuminated from behind by a standard 25-watt frosted bulb placed behind a ¼" aperture, giving a field intensity of 2.7 foot-candles. Settings of the diaphragm opening were read from a scale on the side of the apparatus out of *S*'s view.

The size-estimation method presents a series of standard discs, either blank or bearing a neutral or value symbol; the adjustable circle of light is varied to match the size of the standard disc.

Stimulus-field: As a test of the influence of difficulty of the task, size-estimations were obtained under two experimental conditions. These differed in the extent to which the standard and comparison discs were phenomenally identical and in the extent to which extraneous distracting cues were present. Situation I provided for maximum homogeneity and ease of judgment; standard and comparison discs were essentially identical. In Situation II they were not, and many distracting cues were present. In Situation I *S* sat in a darkened room facing two circles of white light, one containing the symbol, and directed *E*, who adjusted the variable circle to match the standard in size. Presumably under these conditions accuracy should be excellent since the circles were in the same frontal plane and at exactly the same height and darkness reduced the number of extraneous stimuli.

Situation II duplicated the Bruner-Postman conditions and was much more heterogeneous than Situation I. Under full room illumination *S* himself adjusted the variable disc of light to match a pink disc which he held in his left hand approximately one inch forward of the variable stimulus and at a somewhat different height. We might expect that since the compared objects were not identical and since they were compared under less than optimal conditions, accuracy should suffer.

Subjects: If in the present context value influences perception, then *S*s who feel strongly about specific value symbols should show different effects; they should differ from others in the size-estimations they make with these symbols present. To check on this we used three groups of *S*s who presumably differed in this respect. One group of 20 consisted of Menninger Foundation psychiatrists and their wives who were either refugees from the Nazis or who were citizens of German-occupied countries and had resisted the invaders. Another group of 20 consisted of native-born secretaries, employees of the Foundation. It was reasonable to believe that the first group would feel very negative toward the swastika; the perceptual effect, then, of this symbol should be singularly strong for them. The secretaries presumably were less personally involved with the swastika though quite positive toward the dollar sign (these differences in value-intensity were verified in interviews following the experiment). To control the differential effects of age, experience, and education (operating in favor of the refugees), we used a third group of 10 native-born psychiatrists whose age range and experience were approximately equal to the refugee groups.

Stimuli: A possible variable in the Bruner-Postman experiment is the *figural property* of each value and neutral symbol. The "openness" or "closedness," "horizontalness" or "verticalness" of the stimuli, quite aside from their symbolic value, are possible sources of

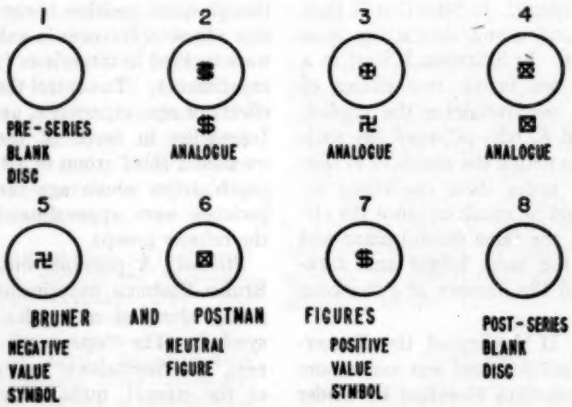
error. To test this we devised analogues for the Bruner-Postman figures. In each one we tried to preserve the figural properties without the value significance of the original figure. Our question was: Will significant differences appear between errors attributable to the value symbols and those to their analogues? The stimulus series, then, consisted of Bruner and Postman's neutral and value symbols, our neutral analogue for each, and a pre- and post-series blank disc. As in the Bruner-Postman study, the figures were one-fourth the diameter of the discs. Each of them was presented on both 1-inch and 2-inch discs to each *S* in either Situation I or Situation II. Three judgments were made of each disc, a total of 48 judgments per *S* in all. The figure shows the stimulus-figures and the stimulus order.

Ten refugees and 10 secretaries were tested in Situation I and the same number of each in Situation II. In addition the group of 10 native-born psychiatrists was tested in Situation II. *Ss* were asked simply to match the standard disc with the variable one. These instructions were straightforward with no special stress on accuracy, and nothing was said about the symbols or the purpose of the experiment until it was over. Then an interview covered the *Ss*' reactions toward the symbols, their associations and attitudes, manner of responding, etc.

Our results are reported in units of $\frac{1}{64}$ of an inch. Thus a score of 5, 10, or 15 represents errors of $\frac{5}{64}$, $\frac{10}{64}$, and $\frac{15}{64}$ inches. Although the amount of error per *S* is extremely small, cumulative errors among *Ss*, groups, and situations are sufficiently great for comparative purposes.

RESULTS

The experimental design provided for a triple classification analysis of variance (7) to test the influences of *intensity of value* (via *"Groups"*), *difficulty of the task* (via *"Situations"*), and *degree of value significance, figural properties,* and mere *presence or absence of figures* (via *"Discs"*). Tests showed, however, that the variances in Situations I and II were not homogeneous, nor were they for the 1- and 2-inch sizes of discs. Therefore, the data were subdivided and six double-classification analyses of variance were set up to test these factors, one for each combination



Stimulus figures in order of presentation.

TABLE 1

TOTAL ERRORS * BY SITUATIONS, GROUPS, DISCS, AND SIZES

Situation	Size	Groups	Pre-series blank	⨀	✢	⊠	卐	⊠	$	Post-series blank	Sum for all discs
I	1″	Refugees	74	21	19	41	37	38	15	60	305
		Secretaries	52	−14	28	24	23	3	7	59	182
		Total	126	7	47	65	60	41	22	119	487
I	2″	Refugees	31	−70	−86	−62	−46	−69	−34	−5	−341
		Secretaries	56	−45	−18	−51	−17	−47	−32	56	−98
		Total	87	−115	−104	−113	−63	−116	−66	51	−439
II	1″	Refugees	103	77	66	9	45	6	15	48	369
		Secretaries	79	63	75	52	80	63	92	97	601
		Total	182	140	141	61	125	69	107	145	970
II	2″	Refugees	131	87	122	80	137	53	92	80	782
		Secretaries	147	109	182	212	204	189	261	249	1553
		Total	278	196	304	292	341	242	353	329	2335
II	1″	Refugees	103	77	66	9	45	6	15	48	369
		Native-born Psychiatrists	127	86	129	111	130	117	115	143	958
		Total	230	163	195	120	175	123	130	191	1327
II	2″	Refugees	131	87	122	80	137	53	92	80	782
		Native-born Psychiatrists	135	129	236	230	276	222	258	243	1729
		Total	266	216	358	310	413	275	350	323	2511

* Expressed in units of $\frac{1}{64}$ of an inch.

of situations and size of disc for the Refugee and Secretary Groups (total, 4) and another pair for the 2 sizes of discs, Situation II, for the Refugee and Native-born Psychiatrist Groups. The basic score in each analysis was the error for each trial (3 trials per disc), taking account of the direction of the error (+ or −) from the true size. Total errors are shown for Groups in Table 1.

Comparison of Refugees and Secretaries: Tested against the *Groups* × *Discs* variance, *Groups* is significant at the .01 and .001 levels for the 2-inch discs. For the 1-inch discs, however, *Groups* is barely significant at the .05 level in Situation I and misses significance at that level in Situation II.[2] Membership

[2] The difference between the Refugees and Native-born Psychiatrists is significant for both sizes of discs with the Refugees more accurate in each case. The Native-born Psychiatrists are not referred to directly hereafter since the control they were expected to afford did not prove necessary. The tables include their data, however, and the discussion of the two experimental groups applies to them as well.

in the Refugee or Secretary Group is, then, not unequivocally significant in its own right but only under certain combinations of sizes of *Discs* and *Situations*. There is interesting support for this in the data themselves in that the Secretaries judge more accurately than the Refugees in Situation I, while the reverse is true by about the same ratio in Situation II. As could be expected, errors for all groups were much larger in Situation II than in Situation I.

Do the two groups react differently to particular discs? For an answer to this we turn to the *Groups* × *Discs* interaction in Tables 2a, 2b, and 2c. This interaction proves to be significant at the .01 level in two of the analyses, at the .05 in another and not significant in the last. In general, *Groups* and *Discs* significantly interact.

To test whether this interaction supported any of the hypotheses of this study, single degrees of freedom corresponding to them were analyzed.[3] Table 3 shows clearly that none of the hypotheses finds support which requires Refugees and Secretaries to differ on valued as opposed to neutral symbols. The two significant F's (in columns 9 and 10) stand alone and are hardly more than could be expected by chance alone.

[3] Note that 12 degrees of freedom are analyzed and these obviously are not all independent of each other, the interaction having but 7 degrees of freedom. Only as many degrees of freedom were analyzed as we had hypotheses about.

TABLE 2a

SUMMARY OF ANALYSES OF VARIANCE

Situation	Groups	Size	Source	df	Sum of squares	V	F	P
I	Refugees and Secretaries	1″	Groups	1	31.52	31.52	1.60**	ns
							6.24*	.05
			Individuals (within groups)	18	876.52	48.70	9.64*	.001
			Discs	7	200.88	28.70	1.46**	ns
							5.68*	.001
			Groups × Discs	7	37.65	19.66	3.89*	.01
			Individuals × Discs	126	636.08	5.05	1.56	.001
			Within Cells	320	1034.25	3.23	—	
			Total	479	2816.67			
I	Refugees and Secretaries	2″	Groups	1	123.23	123.23	11.82***	.001
			Individuals (within groups)	18	1597.14	88.73	8.43	.001
			Discs	7	744.51	106.36	10.20***	.01
			Groups × Discs	7	60.86	8.69	—	—
			Individuals × Discs	126	1325.10	10.52	2.26	.001
			Within Cells	320	1490.66	4.66	—	
			Total	479	5341.50			

* Tested against Individuals × Discs.
** Tested against Groups × Discs.
*** Tested against Groups × Discs plus Individuals × Discs (V = 10.42; df = 133).

TABLE 2b

SUMMARY OF ANALYSES OF VARIANCE

Situation	Groups	Size	Source	df	Sum of squares	V	F	P
II	Refugees and Secretaries	1″	Groups	1	112.13	112.13	5.37** / 12.04*	.10 / .05
			Individuals (within groups)	18	2992.06	166.23	17.86*	.001
			Discs	7	193.10	27.59	1.32** / 2.96*	ns / .01
			Groups × Discs	7	146.09	20.87	2.24*	.05
			Individuals × Discs	126	1173.03	9.31	2.39	.001
			Within Cells	320	1247.40	3.90	—	
			Total	479	5863.84			
II	Refugees and Secretaries	2″	Groups	1	1238.42	1238.42	19.2**	.01
			Individuals (within groups)	18	8441.53	468.97	26.17	.001
			Discs	7	333.66	47.67	—** / 2.66	ns / .05
			Groups × Discs	7	451.55	64.51	3.60	.01
			Individuals × Discs	126	2258.14	17.92	2.72	.001
			Within Cells	320	2104.90	6.58	—	
			Total	479	14828.20			

* Tested against Individuals × Discs.
** Tested against Groups × Discs.

While the groups do not respond differently to value stimuli, it is interesting that 4 of the 6 comparisons of pre- and post-series blank discs are significant (see column 1, Table 3). Also, though the Bruner-Postman figures do not differ from their analogues generally, the dollar sign is reacted to differently from its analogue in 4 out of the 6 combinations of *Situation, Size* and *Group*,[4] for reasons which are obscure to us (see columns 3, 4, 5, Table 3). The three significant differences in column 6 depend, of course, upon this more basic finding. We can only speculate about this, but it does seem possible that the figural difference between the two symbols is responsible for the differences in response, though it is not clear why this should contribute to an interaction variance.

Influence of Discs: A glance at Tables 2a, 2b, 2c, summarizing the six analyses of variance, shows that the significance of *Discs* as a factor in its own right is equivocal. In none of the analyses is it significant when tested against the *Groups* × *Discs* interaction (when that interaction is significant itself). It is significant in all of the analyses only when compared with *Individuals* × *Discs*. Analysis of several individual degrees of freedom[5] of the *Discs* variance (see Table 4) indicates that the valued and neutral symbols

[4] All in Situation II.

[5] These degrees of freedom are not necessarily independent of each other.

(columns 8 and 10) differ significantly in only three of the six comparisons. All three of these involve 2-inch discs, a finding which we are unable to explain. What is clear from all this, however, is that the "accentuation" or error-producing effect of a "value-symbol" is not independent of other field conditions, even of such a condition as size of disc. In column 2, Table 4, we see that the comparison of blank discs with symbol-bearing discs is more consistently significant than those among the various symbol-bearing discs. Apparently, the mere presence or absence of a figure relates to amount of error. Another point: not only is *amount* of error related to other than value factors in the situation but so is the *direction* of error. In every analysis of variance in which the comparison of blank and symbol-bearing discs is significant, the direction of the difference is to "accentuate" the *blank* discs. (See Tables 1 and 4.)

Direction of Error: Considering merely the direction of errors (combined totals for *Groups*) aside from their statistical significance, it is interesting that in almost every combination of size of *Disc* and *Situation* the Bruner-Postman neutral figure (⊠) was judged smallest of the three Bruner-Postman figures and indeed smaller than all or most of the figure-bearing discs, even in one analysis in which all of them were underestimated. Looking in this way at the *Groups* × *Discs* interactions once again, we find that in almost every combination of size of *Disc* and *Group*, the Bruner-Postman neutral figure was again judged smallest. Also, while there was no *general* tendency for Refugees or Sec-

TABLE 2c

SUMMARY OF ANALYSES OF VARIANCE

Situation	Groups	Size	Source	df	Sum of squares	V	F	P
II	Refugees and Native-born Psychiatrists	1″	Groups	1	722.75	722.75	29.27**	.001
			Individuals (within groups)	18	2551.87	141.77	14.12	.001
			Discs	7	179.67	25.67	—** / 2.56*	ns / .05
			Group × Discs	7	172.81	24.69	2.46	.05
			Individuals × Discs	126	1264.89	10.04	2.87	.001
			Within Cells	320	1118.41	3.50	—	
			Total	479	6010.40			
II	Refugees and Native-born Psychiatrists	2″	Groups	1	1868.35	1868.35	25.51**	.01
			Individuals (within groups)	18	7466.33	414.80	17.14*	.001
			Discs	7	449.79	64.26	—** / 2.66*	ns / .05
			Groups × Discs	7	512.56	73.22	3.03*	.01
			Individuals × Discs	126	3049.79	24.20	3.38	.001
			Within Cells	320	2292.52	7.16	—	
			Total	479	15639.34			

* Tested against Individuals × Discs.
** Tested against Groups × Discs.

retaries to overestimate either the dollar sign or the swastika compared to the other, under Situation II the Refugees tended to judge the swastika larger and the Secretaries the dollar sign larger. The Native-born Psychiatrists were equivocal in this respect. Only in this very limited fashion do any of our findings lend support to the Bruner-Postman hypothesis. Neither intensity of value nor the symbolized values themselves influenced the amount or direction of error significantly.

The data caution us further against attributing any striking effects to the valued stimuli: Situation I, the easier task, favored error in the direction of underestimation; Situation II, the more difficult task, favored overestimation. When we categorize all *Ss* according to the amount of positive error or negative error they show, we find that with a criterion of 25 per cent negative error, that is, 12 out of 48 responses being underestimations, then 9 out of the 10 Refugees and an equal number of the Secretaries in Situation I are included. Under Situation II only three of the Refugees and one of the Secretaries exhibit the same tendency. Apparently the *direction* of error as well as its amount depends more upon the particular *method of presentation* than upon the fact of a value being injected into the situation, though we cannot say what factors in the situations were responsible for this directional effect.

It is clear from the analysis of variance tables that both *Individuals*, as a factor, and the *Individuals* × *Discs* interaction are highly significant. No attempt was made to analyze individual degrees of freedom of these variances but we can note in passing the intra-

TABLE 3

SUMMARY OF ANALYSIS OF VARIANCE OF INDIVIDUAL DEGREES OF FREEDOM OF THE *Groups* × *Discs* INTERACTION

	1 Temporal change, pre- vs. post-series blank	2 Blanks vs. all others	3 卐 vs. $	4 卐 vs. ✝	5 ☒ vs. ☒	6 All Bruner-Postman figures vs. all analogues	7 ☒ vs. all other symbols	8 卐 & $ vs. all other symbols	9 $ vs. 卐	10 Bruner-Postman hypothesis $ & 卐 vs. 2 ☒	11 卐 vs. ☒	12 $ vs. ☒
Refugees and Secretaries Situation I, 1″	—	—	—	—	—	—	—	—	—	.10	—	—
Refugees and Secretaries Situation I, 2″	—	.05	—	.10	—	—	—	—	—	—	—	—
Refugees and Secretaries Situation II, 1″	.05	—	.001	—	—	.05	—	—	—	—	—	—
Refugees and Secretaries Situation II, 2″	.01	—	.01	—	—	.10	—	—	.05	—	—	—
Refugees and Native-born Psychiatrists Situation II, 1″	.05	—	.01	—	—	.05	—	—	—	—	—	—
Refugees and Native-born Psychiatrists Situation II, 2″	.01	—	.05	—	—	—	—	—	—	—	—	—

TABLE 4

SUMMARY OF ANALYSIS OF VARIANCE OF INDIVIDUAL DEGREES OF FREEDOM OF THE *Discs* VARIANCE

The columns are numbered to conform with Table 3.

	2 Blanks vs. all others	6 All Bruner-Postman vs. all analogue figures	7 ☒ vs. all others	8 Value symbols vs. all other symbols	10 Bruner-Postman hypothesis 2 ☒ vs. $ and 卐
Refugees and Secretaries Situation I, 1″	.001	—	—	—	—
Refugees and Secretaries Situation I, 2″	.001	—	—	.05	.10
Refugees and Secretaries Situation II, 1″	.01	—	.10	—	—
Refugees and Secretaries Situation II, 2″	—	.10	—	.01	.05
Refugees and Native-born Psychiatrists Situation II, 1″	.01	—	—	—	—
Refugees and Native-born Psychiatrists Situation II, 2″	—*	.10	—	.01	.05

* .10 when tested against within cells variance.

individual consistencies among these within-group differences.

To summarize the main findings:

1. Of the primary factors and interactions only *Individuals* and *Individuals* × *Discs* are unequivocally significant under all conditions. There are significant differences among people in response both to all of the discs taken together and to certain of them taken singly.

2. The significance of *Discs* is much less clear-cut and analyses of several component degrees of freedom offer only equivocal support to the hypothesis that value-tinged discs of themselves tend to be accentuated.

3. *Groups* as a primary factor is also of doubtful significance while the *Groups* × *Discs* interaction is more clearly significant. Analysis of some component degrees of freedom gives no support to the Bruner-Postman hypothesis even when the added factor of intensity of value is allowed to operate.

An interesting fact emerges when we examine individual differences. Classifying all *Ss* according to direction of error, we find there are people in both groups who have stable tendencies either toward over- or underestimation, quite apart from the tendency for Situation I to favor underestimation and Situation II to favor overestimation, and regardless of the skew of the group toward one or the other pole of response. Analysis of all responses which fall 1.96 sigma beyond the mean error reveals that the bulk of these responses was produced by one or two people; the remainder of the group clusters about the mean. Though we have not evaluated the significance of these preferred modes of response for the individuals concerned, conceivably they may reflect differences in ego structure, expressed through stabilized,

preferred perceptual attitudes. We will discuss this point further in a later section.

DISCUSSION

a. *Comparison with Bruner-Postman results:* The only important factor in our experiment which has special impact on error is the difficulty of the perceptual task—the less optimal the conditions for judgment the greater the error—a finding which seems quite acceptable on its face and apparently requires no involved explanation. None of the three groups responded in any consistent way to particular value symbols as such, even though the intensity of the value, particularly toward the swastika, differed widely among them. There is, of course, the weak directional effect among the Bruner-Postman figures as noted above. But this is not a consistent finding, and in relation to their neutral figure the interpretation is just as tenable that the neutral figure, because of its "closed" figural properties, tends to make the disc on which it appears seem smaller than those on which more "open" figures appear.

The tendency for the more difficult condition (Situation II), which was also the Bruner-Postman condition, to favor overestimation may help to explain why their findings are in this direction. But note that all three of their figures, neutral one included, were overestimated in their study. This raises the puzzling question: Why, if values dominate perception, should the *"neutral"* figure also have been overestimated? Factors other than value must have influenced error in their experiment too. Carter and Schooler (3) also report a constant and unexplained error. Actually value alone can explain comparatively little of the *total* amount of "accentuation" seen in the Bruner-Postman results. If one considers the relative amounts of overestimation which they report for their three figures, one notices that the overestimation of the neutral figure (from zero error) is *proportionately greater* than the overestimation of value symbols from the neutral figure. The additional error which could be attributed to value alone even in their results is rather small. At any rate, we were unable to repeat their results save in the tendency of our common method of presentation to favor overestimation.

In our Refugee Group were *S*s who showed a violent antipathy for the swastika to the extent of upbraiding *E* for having the poor taste to display it even in an experiment. Yet among these, too, the swastika had no greater effect on error than any less-valued symbol. Thus, even where value was demonstrably present and of high intensity, it was not effective in distorting judgment.

Our own findings seem to question whether values in general are of any consequence to perception in general, at least under our experimental conditions, and suggest that the proper locus for study is the individual and how he is constituted to cope with values, among other stimuli. But Bruner and Postman (2), Bruner and Goodman (1), and Carter and Schooler (3) do find in common that the sizes of valued objects *may* be overestimated (though they disagree as to the conditions of this effect). Our findings, under certain, though not all, of our experimental conditions, give mild support to this most general proposition. Apparently there are enough data to justify such a minimal conclusion, but it is obviously necessary to reformulate the problem and to search for the hidden variables and special conditions responsible for the effect.

In view of the widespread interest in the present problem and the great differences in experimental findings, we might reconsider the assumed relationship of values to perception, the adequacy of the size-estimation method

used here for studying it, and some variables, heretofore not made explicit, which could help us to understand the disparate findings.

One thing is certainly clear: The accentuation hypothesis is overly simple. There is in it the danger of being seduced into assuming that quantitative accentuation expresses the totality of relationship between value and perception, that this is the *only* way of registering value perceptually. Lest this implication hide itself in our thinking, we must realize that the hypothesis is a product of the special circumstances of a particular method selected for convenience and ease of measurement. The model of our own experiment is a case in point. Out of the great complexity of the experimental situation in which we place *S*, we select two data, amount and direction of error, to represent what has occurred. It is a procrustean requirement that the subtleties and complexities of ties between values and perception should be lopped off or stretched to fit the requirements of easy measurement. But our method forces us to do one or the other and leaves open several embarrassing questions: Can we be sure that values have affected *only* those who show a constant error of judgment? If not, where must we look to discover why some *S*s overestimate and others underestimate, while some do neither even when experiencing a value strongly. Are value effects *inevitably* reflected only in magnitude of error? It seems more profitable to forego the search for explanatory principles in strictly motivational terms and to see the problem of needs and perception within the framework of a more general psychology of ego structure. We will try in what follows to do just this.

b. *"Task-relevance" as a variable:* Many of our *S*s wondered if the presence of the symbols might affect their judgment of the surrounding disc. But this expectation was not borne out; all *S*s found it quite possible to ignore the symbols. This calls into question the *relevance* of the values and value-symbols to the judgment required of *S*.

In the present experiment, as in its model, the value symbols and even the values themselves were really irrelevant to the task which faced *S*. Rather than serving as a guide or providing a "set" or frame of reference to aid in the purposeful selection and organization of stimuli, the value we hoped to arouse was imposed on the task; it was frankly expected to disrupt it, to have an error-producing effect. This is not the inevitable role of values or needs in perception. For instance, when we are hungry and look for food, the need is a guide to the effective use of perception. Similarly, when we enjoy a work of art or view it critically, values frame our perceptions. But in the present context the values we grafted on to the task could have had no function and like other aspects of the stimulus-situation which were irrelevant and extraneous had to be ignored in favor of more relevant cues. It is reasonable to ask if and how people differ in the ease with which they can ignore pressing, though irrelevant, cues in favor of more relevant ones, and studies of this are now in progress in our laboratory.

There is a difference in this respect between the "coin" experiments of Bruner and Goodman (1) and Carter and Schooler (3) and the "symbol" experiments of Bruner and Postman (2) and the present authors. The value of a coin is far more "imbedded" in its size than is the value of our swastika in the size of its disc. In the latter case the disc has no required tie to the symbol but merely contains it; it is stuck on ornament-like. Why should we expect the swastika to influence the size of a surrounding disc to which it has no intrinsic tie? The relevance of the

swastika might change of course, were the disc itself assigned a meaning consistent with it, as, for example, if it were called a Nazi "medal." Experiments to test the importance of "task relevance" should observe this distinction.

As an aside, we could expect, if this reasoning is correct, that for individual *S*s with greater or lesser ability to exclude irrelevancies from the size-estimation task we should find greater or lesser amounts of error attributable to the impact of the value. Of course this factor alone would not tell us why errors should be toward overestimation (accentuation) rather than underestimation. To predict the particular effect of a value for a given *S* it is not enough merely to know the conventional value he attributes to the stimulus-object and the intensity of the value; it is equally important to understand the way he is organized to perceive and to cope with values. It is simply not sound to hypothesize a fixed or constant relationship between the presence of a conventionally valued stimulus in the stimulus-field and distortion, in a particular direction, of perceptual behavior.

c. *Relation to perceptual "constancy":* With the notion of "relevance to task" as a bridge it is useful to regard our experimental task as essentially one which requires *S* to maintain object-constancy. *S* is guided by *E*'s instructions to confine his attention to a particular attribute of the object (in our instance, size). His comprehension of these instructions helps *S* to develop his own range of equivalence for the objects judged. Let us look at this term "equivalence." When *S* compares two objects for size he judges whether they are equivalent in this respect alone. The judgment of equivalence is not only a function of the actual sizes of the objects compared but depends as well on *S*'s purpose in making the comparison—his intention toward the task, the "meanings" he gives to the objects. For instance, objects of identical meaning are likely to be seen as "same" while those with different meaning will tend not to be so; an adult and a child so placed that their retinal images are of the same order of magnitude will not usually be judged to be of the same size. Besides intention, another factor which affects equivalence is the physical conditions of judgment.

The range of equivalence, then, is that distance along a subjective scale within whose limits sizes do not appear to be different. That equivalence is not solely a matter of comparing proximal stimuli for size can be seen in many everyday situations. The criteria used for judging size vary with intention, and objects are accepted as equivalent if they are "big enough" or "small enough" to suit this criterion—*i.e.*, if they will fit. For some purposes wide variations in object size (or proximal size) will still evoke a judgment of "equal" while for other purposes small variations will be inacceptable and lead to a judgment of inequality. Thus the range of tolerable size-variation is a function of the "purpose" for which the judgment is made.

Conditions of judgment also affect the equivalence-range directly; how much they will do so will partly depend upon the prevailing intention. But they may also bring about a modification of the intention and in this way affect the equivalence range. Usually when high accuracy is required, we try to arrange the most optimal conditions for achieving this. We move the compared objects into the same light; we hold them at the same distance; we may squint a little bit to cut out background stimuli, etc. But it is often impossible or impractical to manipulate conditions to satisfy an intention of high accuracy. In such circumstances two things can happen: Since it is harder to keep in-

trusive and irrelevant cues from defeating the goal of high accuracy, the intention itself may change and become somewhat more modest to suit the conditions. As a result a sort of "compromise" intention develops; *S* strives for a degree of accuracy which seems "reasonable," and his expectation is for lessened accuracy. He now has a wider range of equivalence. In general, it is safe to say that the equivalence-range —the amount of difference we can tolerate without an experience of inequality —varies with the intention of the moment and is tempered by conditions of judgment.

Of course, real disparity could exist between what *S* feels he is required to do and how easily he can adopt a correspondingly appropriate intention. This takes us into questions of ego structure and its counterpart in perceptual organization. What does *S* find comfortable to do, what limits are set for him by his ego structure when he grapples adaptively with a situation? We will deal with these questions later on, but in any case, what *S* will take into account most of all in our Situations will be what he deems most relevant to the task as he is able to interpret it, and as *E* encourages him to interpret it by instruction, and as the conditions for judgment permit. He will neglect, or try to neglect, other facets of the situation to the degree that his perceptual organization permits him to be selective and discriminating.

It should be easier now to empathize with *S* in our experiment. In Situation I he faces a stimulus-field in which two circles appear identical save for size; *E* directs him to attend only to size, or rather to differences in size. To carry out *E*'s instruction to the letter *S* tries to discriminate small differences. His over-all orientation is one of *directed attention*. Under these circumstances his "set" will be for accuracy or precision, and he will try to exclude all irrelevant cues, even though instructions may not explicitly require this attitude. We can ask: What sets the limits of *S*'s tolerance for error? Here is where "constancy" theory can be useful. If *S* would be maximally "accurate," he will make his judgments as nearly as possible in terms of proximal size cues alone and will strive for what we have called a narrowed range of equivalence. Situation I favors this "set," and in fact, the dark room conditions, the absence of distracting cues and objects, the total emphasis upon the most optimal conditions to enable *S* to direct attention upon size alone—all these factors make this situation not unlike the reduction-screen field of constancy experiments; the intended attribute (size) is fully conveyed by the proximal stimuli alone. Of course, Situation I does not provide the sort of conditions for which "constancy" explanations are usually offered; here proximal stimulus-size is also the best possible cue to object-size; no "compromise" is necessary. We would consider Situation I, however, a sort of end pole for constancy-theorizing, for which Bruner and Goodman's memory situation (comparison of *memory* of a coin with a series of standard discs) might represent the opposite pole. Errors of size-estimation in Situation I reflect, then, how accurate *S* can be in judging proximal cues.

When the task is complicated, as in Situation II, by differences in color, distance, brightness, etc., more cues become relevant and necessary to consider. Unlike the funnelled, simplified field of Situation I, irrelevant cues now make a greater bid for attention; they become more intrusive. In such circumstances the range of equivalence widens as *S* is forced to weigh multiple cues in order to maintain high object-constancy. Where before it was enough to consider only proximal size cues as an indicator

of object-size, now he has to take account of distal factors of color, distance, and brightness to make the required judgment. Perhaps we can consider the imposed value of our experiment as still another insistent but irrelevant stimulus, which *S* ought to ignore, a discrimination which he may have trouble in making.[8] This would help us account for greater error in the presence of potent value stimuli (among other temporary but significant states of the person) in those cases where it might occur.

Note that the increased range of error, which seemed so self-explanatory when we invoked "difficulty of the task" as the only relevant variable, can take on additional meaning when the size-estimation task is viewed in the context of constancy, equivalence, and intention.

But what of the direction of error, that is, the tendency for errors to be either overestimates (accentuation) or underestimates? Our explanation has covered only the fact of increased error. The Bruner and Postman contention that valued objects are necessarily subjectively enlarged is based entirely on group averages. Our own data show that such averages combine the data of those who consistently overestimate and some who just as consistently underestimate. It is probably an oversimplification, therefore, to assume a unidirectional effect. Then, too, it seems unlikely to us that value influences can ever be capable of precise description without reference to the particular perceptual organizations and ego structures of the perceivers who experience the value. That the factor of ego organization, as well as sheer intensity of value, is crucial in predicting value-effects is implied by our finding that the errors for non-valued as well as valued stimuli tend to be consistent for the person. Preferred perceptual attitudes, which show themselves in particular situations, should certainly be a center of inquiry.

Besides the factor of individual propensity, another possibility is that difficulty in judgment, rather than the compelling force of strong value, may encourage overestimation. An increased group tendency toward overestimation appeared in Situation II and this was non-specific for the symbols.

d. *"Preferred perceptual attitude"* (Anschauungen) *as a perceptual variable:* Elsewhere we have described a basis for viewing perceptual organization in the context of ego psychology (5, 6). Here we can only restate our argument in a general form: Similar characterological principles govern the functioning of all aspects of a person's subjective field. In this sense, perceptual organization bears the stamp of ego structure. The dimensions in which are organized a person's perceptual experiences are counterparts of those which structure his experiences in other spheres, whether motor, affective, or cognitive. In perceptual functioning we regard these dimensions or characterological principles as *preferred attitudes* or *Anschauungen* which give particular qualities to a person's perceptual experience; they guide the selective and protective functions of perception.

We have in mind this broadened meaning of "preferred attitude" as a perceptual dimension of ego structure when we say that it can influence the degree of intrusiveness of irrelevant cues (including value and need) and perhaps even the direction of their effect. Two people may differ in how easily they can ignore irrelevancies, whether these are solely in the perceptual field or whether they involve motivational factors of value and need. Because of differing "preferred attitudes" which control per-

[8] Experiments are now in progress to look into the role of "sensory fusion" as a source of error. Perhaps tactual and kinaesthetic impressions intrude upon the use of visual cues, thus increasing error.

ceptual organization, some *Ss* may be unable to minimize the influence of irrelevant factors upon judgment and to prevent a "sensory fusion," even when recognizing certain factors as irrelevant. Direct evidence on this point is scanty, but the literature is suggestive enough for us to urge that the factor of "preferred attitudes" of perceptual organization requires recognition in experiments in this area.

In the Bruner and Goodman, and Carter and Schooler experiments the *Ss* were children. Werner (8) and others agree that children, like "primitives," favor a more "syncretic," or synaesthetic mode of perceptual organization in which all factors in the perceptual field influence the perception of any one aspect of it. It is as if for them the lines between figure and ground of situations are thinly drawn and easily permeable. Jaensch (4) has termed a similar form of perceptual organization an "integrate tendency"; Werner's syncretic vs. analytical modes seem also to refer to this dimension of perceptual organization. Among adults in general whose perceptual field, in Werner's terms, is more "differentiated," we might expect it to be easier to abstract relevant attributes, maintain them stably as "figure" and thus make accurate judgments. It would not surprise us, then, to find that the effects reported by Bruner and his colleagues would be more evident in children than in adults.

But we need not look to perceptual organization merely as a vestigial or rigidified residue of development; we may concern ourselves too with its present adaptive significance as well. The perceptual system selects and orders stimuli in such a way as to permit selective response and prevent the person from being overwhelmed by the continuous welter of outer and inner stimulation. We may call this process "defense." With such a view of the perceptual system we could expect that individuals would develop characteristic and idiosyncratic ways of accomplishing their adaptive ends perceptually. We would regard "preferred perceptual attitudes" as the counterpart in the perceptual sphere of the idiosyncratic adaptive "habits" worked out by the person. We might find that the large and often reliable individual differences which such experiments as ours report could be understood in terms of such personal but consistent ways of coming to terms with adaptive tasks in the perceptual sphere.

Of course, it would be a serious matter indeed for the person if an irrelevant cue, even a value stimulus, could disturb perceiving to any great extent. Such an occurrence might be an index of pathology, or, in any case, of a very different kind of perceptual organization, whether temporary or permanent, from that which we call "reality oriented." We should take note that the errors reported by Bruner and Postman as significant by-products of value and need stimulation are, *in an absolute sense,* quite small and to a casual observer rarely dramatically "out of line."[7]

e. *Person vs. stimulus-field: Implications for method:* Our discussion of critical variables in value and need studies has led us to emphasize "perceptual organization" and "preferred attitudes," a move which has important implications for the methods used in such studies. If people differ in how easily they can ignore irrelevancies, clearly our methods should give free play to this variable. But, this would mean that we center attention not solely on the situation but on the *person* as well. How is he organized to cope with his values and needs? How well can he

[7] "Memory experiments" such as Carter and Schooler's, which report large errors (3), permit *S* to make no direct comparison with "reality."

select what is "appropriate" to the task at hand? Our results suggest that consistent overestimation or underestimation may be typical for certain variants of personality organization. However, the explanations which the present type of experiment is forced by its design to offer, ignore the person as a source of variation and must be content to draw relationships between the presence of a generalized value-stimulus and a generalized response. We are in the position of dealing with value in the absence of any *valuer*.

The method we used required groups of *Ss* whose personalities and perceptual organization were unknown to us. It also required the combining of all of their responses, as if all had the same single meaning. In effect, it removed any possibility of learning how any *person*—the locus, after all, of the value itself and of the perceptual act—reacted to the experimental situation. As we have pointed out, the group response combines the responses of those who underestimate and those who overestimate. Such an average might differ insignificantly from that of any other group even though within the groups there might be dramatic and reliable differences. Could we rightly conclude from comparing two such averages that values have no influence on perception? Certainly not, for it is our method which dictates these results. What we can safely say is that if values do influence perception, then this method is by its very design impotent to reveal it. In assuming unwittingly that the situation is the same for all persons and that all persons react equivalently, we limit ourselves to relating generalized field conditions to generalized behavior. Such diluted generalizations in situational, rather than personality, terms no doubt have their place in some areas of psychology and for some kinds of problems. But we doubt very much if they are at all suitable in the areas of personality and motivation or in perception as it bears on either of these. Somewhere in the statistical shuffle the person has been lost.

SUMMARY AND CONCLUSIONS

This experiment was designed to evaluate the Bruner-Postman "accentuation hypothesis" for describing the influence of values on perception. It had also a methodological intent: to examine the adequacy of the size-estimation method from which their generalization issued.

Using their method, the present study weighed the influence of four additional variables upon errors of size-estimation: (1) intensity of value, (2) difficulty of the size-estimation task, (3) figural properties of the value-symbols, (4) gross presence or absence of any figure, valued or neutral. As in the Bruner-Postman study, the Nazi swastika and American dollar sign were the value-symbols. European refugees (high value-intensity toward the swastika) comprised the experimental group and they were compared with native-born controls (lower value-intensity toward the swastika and high value-intensity toward the dollar sign).

The main findings were:

1. The differences in error among the *Groups* could not be unequivocally attributed to the effect of value.

2. For each group, the error magnitudes for value figures and for neutral figures were insignificantly different. Therefore, *intensity* of value and degree of value significance had no consistent influence upon error.

3. The factors which grossly influenced error were difficulty of the task, and size of disc. The less optimal the conditions for judgment and the larger the disc, the greater the error.

4. Difficulty of the task also seemed to have a *directional* effect upon error: the more optimal conditions seemed to favor underestimation while the less optimal conditions (equivalent to the Bruner-Postman conditions) favored overestimation.

5. Certain *Ss* in both groups showed consistent tendencies either to overestimate or underestimate and these seemed to be independent of particular stimulus-figures.

A critique is offered of the method of this experiment. The divergence of present results from other reported findings, and the necessity of giving choice of method an explicit conceptual basis, argue for re-conceptualizing the problem of values and needs in perception, and rooting it in the study of individual variation, perceptual organization, and ego structure rather than solely in terms of motivational theory. The present experimental situation is seen as a variant of the "thing constancy" problem in which personal intention is a crucial factor. Possible ways are discussed in which stabilized, "preferred perceptual attitudes" might affect intention and hence errors in such situations.

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AUTONOMIC DISCRIMINATION WITHOUT AWARENESS: A STUDY OF SUBCEPTION

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Introduction

This paper is a report of an experiment which extends and supports findings which we have previously described in an interim report (13). The results indicate that at tachistoscopic exposure speeds too rapid for correct recognition, subjects are able to give discriminatory responses as measured by their galvanic skin response (GSR). This perceptual process was called by the authors *subception*. The data also suggest that it is important to control for the verbal response preferences of the subjects when evaluating their perceptual accuracy.

A number of considerations prompted the coining of the new term, subception. Despite the inadvisability of recklessly increasing the "deadwood" in the psychological vocabulary, we know of no other term that precisely defines a *process by which some kind of discrimination is made when the subject is unable to make a correct conscious discrimination*. We rejected "subconscious perception" (the only standard terminology that occurred to us) for several reasons. In the first place, the word "subconscious" or "unconscious" is replete with controversial implications, none of which are pertinent to the present case. We particularly wished to avoid the psychoanalytic notions concerning the subconscious, and the alternate notion that the subconscious is one level of awareness —on the inattentive fringe of consciousness, so to speak. Secondly, although there is less quarrel with the word perception, it too often implies awareness on the part of the perceiving organism. In fact, three of the four definitions of perception in Warren's dictionary use the term "awareness." Even if one believes that awareness is not necessary for perception and thus regards subception as simply a special case of perception, we feel that it is a sufficiently unique case to require a unique term. There is, of course, some intrinsic value in using a single word rather than a more cumbersome phrase to identify this perceptual process.

The original impetus for this study came from the recent interest in the role of needs in perceptual behavior. It has been reported frequently that stimuli of different need significance may have different recognition thresholds (1, 6, 10, 12, 14, 19, 20, 24). Although theorizing in this field has lacked precision and completeness, it is possible to find two general frames of reference which have been used to interpret the "need in perception" observations. The first might be identified as the "response availability" approach. The second could be termed the "dynamic" point of view. Both views are by no means incompatible.

Writers who have preferred to use the concept of response availability have pointed out that, for different individuals, some words have greater frequency of occurrence than others (2, 7). Differences in this response availability could act in two ways to produce differential recognition thresholds, depending in part upon the degree of ambiguity of the stimulus material. On the one hand, the subject is more likely to make use of minimal cues from words which

are more readily at his disposal than from those which are not. This aspect of the concept appears to be very much like the old notion of attention or set. On the other hand, if the cues are so minimal that the subject appears to be guessing, the presence of certain words in his response repertoire will increase the statistical probability of these words being correctly identified.

Other workers, notably the clinically oriented writers, have implied the unconscious participation of the individual in actively selecting and rejecting the presented material in accordance with his needs (6, 8, 10, 14, 15). For example, McGinnies (14), summarizing some of the work in this area, states, "It seems well established, then, that the perceptual 'filtering' of visual stimuli serves, in many instances, to protect the observer as long as possible from an awareness of objects which have unpleasant emotional significance for him." In elaborating this type of approach, Eriksen (6) and Lazarus *et al.* (10) have talked about such variables as type of ego defense and the acceptability of the need—factors which, they believe, can influence the degree to which the subject is able to verbalize and recognize the stimulus material.

Whatever the merits of these two points of view may be, the latter approach, which we have loosely called "dynamic," places its proponents in the difficult position of having to postulate some process of discrimination occurring prior to the ability of the subject to report correct recognition. More specifically, if the observation that a subject can recognize the word "sacred" at faster exposure speeds than the word "income," is attributed to their differential need value, it would be necessary to assume that the subject is somehow identifying the significance of the two words before he is able to report recognition of them. The purpose of the present experiment was to attempt to test this assumption. We have asked the question, "Can subjects make discriminatory responses even when they are not able to report the stimulus correctly?"

There have been other attempts to get at the problem of "discrimination without awareness." Miller (16), in a paper reporting an experiment of his own, reviewed investigations of this problem over a period extending from 1863 to 1938. Miller's experiment, and all the studies he mentioned, differ from the present research in some important ways. For example, Miller showed that accuracy of discrimination was better than chance below the subjects' "limen of awareness." His criterion of discrimination was the correctness of the subject's verbal statement. In the present experiment we are asking whether a discrimination can be made when recognition, as defined by a correct verbal report, is impossible. The problems are therefore not the same. A second major difference in the Miller experiment lies in the fact that whether or not a subject was actually performing below his "threshold" was entirely a subjective and statistical matter. The subjects' limens were obtained by the method of limits. In the present experiment, as you will see, a subject is judged to be operating below his "threshold" when his verbal report is wrong. The danger in Miller's approach is emphasized by the earlier findings of Perky (18), who showed that subjects could not tell the difference between real images which were slightly supraliminal and impressions which were imaginary.

There are several other experiments which are relevant here because of their use of conditioning procedures or the galvanic skin response. A novel approach has been used by Redlich (21) and Levine (11). By the use of hypnotically induced anesthesia, or patients

with hysterical anesthesias, these investigators showed that subjects gave psychogalvanic responses to stimuli applied to an anesthetic area. Scott (22) conditioned a finger-withdrawal response during a trance state and tested for the persistence of the conditioned response during the post-trance amnestic period. Presenting mean scores for the eight subjects, he concluded that there was some residual conditioning in the post-trance period, even though the subjects remembered nothing of the conditioning trials and consequently were not "aware" of the significance of the conditioned stimulus.

A different line of attack has been the attempt to condition responses to stimuli which, by various criteria, are below the subject's psychophysical threshold. Silverman and Baker (23) used subliminal alternating current as the conditioned stimulus and paired it with several kinds of emitted responses in human subjects. Although the authors saw some evidence of eye-wink conditioning in three out of ten subjects, the results do not warrant positive conclusions on a statistical basis. Newhall and Sears (17) conditioned finger-withdrawal to a supraliminal light stimulus and tested for conditioned responses at and below the psychophysically determined limen. They reported obtaining conditioned responses with stimuli which were below the limen, and "in several instances visual stimuli that were individually reported unperceived, had evoked the conditioned response." This latter observation was incidental to the main interest of the experiment, and Newhall and Sears stated that they believed the problem of whether a conditioned response could occur without consciousness of the stimulus was still open to question.

In this vein it might be possible to have the conditioned stimulus well above the subject's sensory threshold but, by proper training procedures, have the subject unaware of the fact that the stimulus was a conditioned one. This was the idea behind the work of Diven (5). He used the GSR for his response and masked the conditioned nature of the stimulus by applying the unconditioned stimulus after a 12-second delay. During the delay the subjects free associated to the conditioned stimulus. Most of the subjects reported no awareness of the relationship between the conditioned and unconditioned stimulus, and yet showed conditioned GSR's. Since the confounding effect of stimulus generalization in this experiment cannot be evaluated, Diven's results, showing conditioning without awareness, remain inconclusive.

A more recent study which attempted to demonstrate discrimination prior to correct recognition was reported by McGinnies (14). Using tachistoscopic presentation and employing a method which was in some respects similar to the present study, he found that subjects gave GSR's which were greater for emotional words than for neutral words, before the words had been consciously recognized. A crucial shortcoming of the experimental design was that the subjects could have been motivated to withhold their report of the socially taboo words (such as *whore, bitch, raped,* etc.) even after some suspicion of their meaning was present. In other words, the GSR during the pre-recognition trials could very well have been an emotional response to recognized words which were not yet reported.

This matter of withholding of reports was treated at some length by Howes and Solomon (7), who also attempted an interpretation of McGinnies' findings based on their notions of word-frequency as a determiner of perceptual accuracy. We have briefly discussed this kind of theoretical orientation under the heading of "response availability." McGinnies (15), in a

reply to the note of Howes and Solomon, handled the specific word-frequency criticisms adequately but was unable to explain away the possibility that the subjects were motivated to withhold their report of the taboo words. McGinnies wrote, "It must be admitted that this possibility does, in fact, constitute one of the knottier problems in this kind of research." The experimental design in our experiment was planned to unravel this knot, because the present authors are in agreement with Howes and Solomon that the danger of withholding responses is indeed crucial to McGinnies' conclusion.

Considering the approaches used and the data reported, the authors believe that previous research has not satisfactorily demonstrated that a process of discrimination can operate prior to conscious recognition and in the absence of the possibility of the correct verbal report.

PROCEDURE

In the present experiment, we recorded the subject's GSR by means of an AC bridge apparatus.[1] Dry silver recording electrodes, ¼" square, were attached to the right forefinger and right middle finger tip of the subject. GSR's were read directly as peak deflections on a Ballantine Electronic Voltmeter. These readings were converted into actual impedance changes by substituting a variable resistance box in the circuit in place of the subject, and matching the Voltmeter readings obtained when the subject was in the circuit, with the phase angle of the bridge set at zero. We could read pure impedance change because the AC bridge was designed so that the subject's apparent capacitance change could be balanced out during the experiment by operating a dial that kept a Lissajous figure closed when the phase angle of the subject and the bridge were exactly equated. This impedance change (ohms) was then transformed into admittance change (mhos) in keeping with physiological (4) and statistical (9) suggestions as to the most meaningful unit of measure for the GSR.

We presented the stimulus words on a beaded screen seven feet from the subject by means of a projection tachistoscope. This arrangement allowed for a variation of exposure speeds from $\frac{1}{150}$ second to one second. The illumination and range of exposure speeds varied from subject to subject depending upon his perceptual performance and, once established, were held constant for each subject. Five different exposure speeds were selected for each subject such that the slowest speed resulted in near 100 per cent accuracy of recognition of the syllables. In all cases, the fastest exposure speed resulted in accuracy of recognition which did not differ significantly from chance.

Five-letter nonsense syllables were used as the stimuli to minimize as far as possible differences in familiarity. Moreover, the use of nonsense syllables precluded the possibility that subjects would have any motivation to withhold their report. This was, you will recall, a crucial inadequacy in the experiment by McGinnies (14). Ten syllables were presented: YILIM, ZIFIL, GAHIW, GEXAX, JEJIC, JIVID, YUVUF, ZEWUH, VAVUK, VECYD.

The procedure for each subject can be divided into three parts.

1. *Equation period.* After practice at recognition we presented each subject with the ten nonsense syllables tachistoscopically. The syllables were randomized for both order of presentation and exposure speed. Even at speeds where accurate recognition was

[1] The authors are indebted to Thomas G. Arnold, at present in the Department of Medicine, The Johns Hopkins Hospital, for the design and development of the GSR apparatus used in this research.

impossible, each subject was required to make a choice verbally from the ten syllables which he knew made up the stimulus material being presented to him. To aid in this choice each subject used, in succession, ten different lists of the syllables. Each list was in a different order to preclude list-order preference on the part of the subject. After 100 presentations (each of the ten syllables having been flashed twice at each of the five exposure speeds), we divided the ten syllables into two groups of five each, equated for both the number of times the subject used the syllables in the entire 100 responses and the number of times they were correctly recognized. In all cases it was possible to make this two-group division with a high degree of equality. Also, in this phase of the experiment, we noted the exposure speed at which near 100 per cent accuracy of recognition occurred, and used it as the slowest speed in the final test period.

2. *Conditioning period.* In this part of the experiment we employed a one-second exposure speed for all syllable presentations. The GSR was conditioned to the five experimental syllables using electric shock as the unconditioned stimulus. Partial reinforcement was employed, with one-third of all presentations of the five experimental syllables being shocked in random order. During this conditioning period each of the ten syllables was presented an equal number of times to prevent unequal familiarity. This procedure was continued until consistent conditioned responses to the five experimental syllables were established. We instructed subjects not to report during the conditioning trials but merely to identify to themselves the syllables presented to them. This was done to avoid associating the shock with the subject's verbal report and thus influencing his response preference during the final test period. We instructed the subjects at the beginning of the conditioning trials that when they were shocked it would be two or three seconds following presentation of the experimental syllables. This allowed us to read the GSR before the shock was applied. They were further instructed that they would never be shocked after any presentation of the five control syllables.

3. *Final test period.* The procedure of random presentation of the syllables used during the Equation Period was repeated here. The exposure speeds were set so that the subject had near 100 per cent recognition at the slowest of the five exposure speeds. This time, however, GSR's were recorded during the time between the tachistoscopic flash and the subject's verbal report. We told the subject to delay his report until signalled, which was about five seconds after the tachistoscopic flash. This delay was used to prevent the verbal report from contaminating the GSR, and the signal was not given until the GSR was recorded for each syllable presentation.

During this final period no shock was paired with any of the syllables. Because of the rapid exposure speeds used in this period, we had feared that reinforcement at this point would result in extinction of the clear-cut GSR discrimination acquired during the Conditioning Period. This inability to use reinforcement in the final phase, however, created no special difficulties. As a result of the instructions and the prior experience with partial reinforcement, the subject's expectancy of being shocked—and consequently the "conditioned GSR"—was maintained throughout the syllable presentations without further use of shock. We had found with preliminary subjects that in the absence of this continuing expectancy of shock, the GSR promptly disappeared. This is in keep-

ing with the findings of Cook and Harris (3).

The following raw data were recorded during the Final Test Period:

1. The syllable flashed.
2. The exposure speed of the syllable.
3. The subject's verbal choice from the ten possible syllables (whether or not he was correct and whether he reported a "shock" or whether he reported a "non-shock" syllable).
4. The subject's GSR for each syllable presentation.

RESULTS

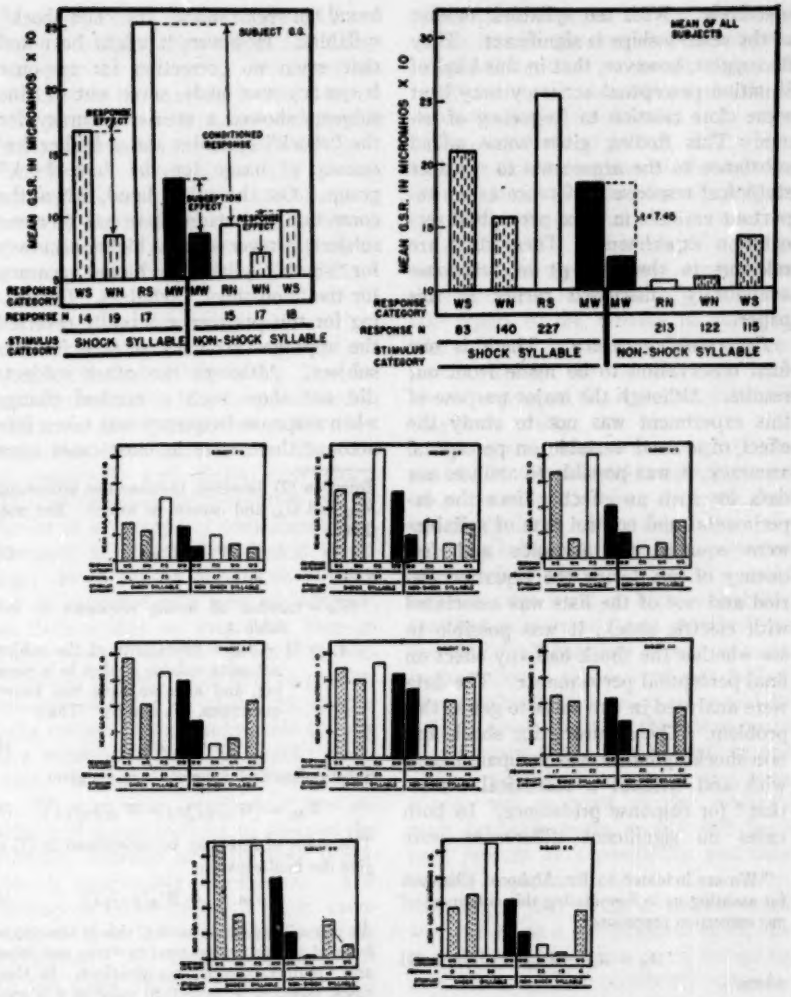
The results of this experiment can be discussed under three main headings: (1) The galvanic skin responses during the Final Test Period; (2) the relations between response frequency and the accuracy of report for various syllables; and (3) the effect of electric shock on the perceptual thresholds.

GSR data. By far the most striking finding to be reported here concerns the autonomic activity of the subjects in the Final Test Period. These GSR data are shown in the bar graphs in the figure. The average GSR's are put in various columns on the basis of certain criteria. The first main division is dependent upon whether or not the syllable flashed had been associated with shock during the Conditioning Period; this is the Stimulus Category ("shock" vs. "non-shock"). The other breakdown is based on the nature of the subject's verbal report; this is the Response Category. There are three kinds of responses in each of the two stimulus categories: "WS" means that the report was wrong and a "shock" syllable was given by the subject; "WN" indicates that the report was wrong and a "non-shock" syllable was given; "RS" and "RN" signify responses of the appropriate stimulus category which were right. "Response N" refers to the number of separate GSR's contributing to the mean for each column.

The two "MW" (mean wrong) bars are the important GSR measures to note. These represent the average GSR of the "WS" and "WN" categories for each of the two types of stimuli. Therefore these "MW" columns give the average GSR (equated for both "shock" and "non-shock" responses) when the subject was not able to perceive (*i.e.*, report) the flashed syllable. It was necessary to average the GSR's associated with wrong responses because the verbal report itself had an effect on the GSR. You will notice that this "response effect" can be seen in the bar graphs as larger GSR's when the subject used a "shock" syllable as his report. By averaging "WS" and "WN" categories, this "response effect" was effectively controlled.

In every subject the "MW" column is larger when the stimulus presented was a "shock" syllable, and you will recall that in this "MW" category the subject was not able to perceive the syllable correctly. This is the *subception effect.* It is summarized in the bar graph entitled "Mean of All Subjects." Student's "t" for this difference is 7.45, which is significant far below the one per cent level of confidence for eight degrees of freedom. The subception effect was found at all five tachistoscopic exposure speeds when wrong responses were made. It was smallest at the speeds which resulted in either very low or very high accuracy. The systematic increase in the effect as a function of decreasing exposure speed which was suggested in the interim report was not substantiated with further data. *There seems to be little doubt that subjects can make autonomic discriminations when they are unable to report conscious recognition.*

Response frequency. Since this experiment provided a limited multiple-



GSR data from final test period.

choice response situation for the subjects, it was possible to obtain some information about the importance of response frequency in relation to perceptual accuracy. A product moment correlation was obtained between the number of times each syllable was used and the number of times it was correctly reported. This was done separately for both the Equation Period and Final Test Period data. Statistical independence of the arrays was achieved by subtracting the number of times each syllable was right from the number of times it was given as a response. These correlations were + .61 and + .67 re-

spectively. With ten syllables, neither of the relationships is significant. They do suggest, however, that in this kind of situation perceptual accuracy may bear some close relation to frequency of usage. This finding gives some added substance to the arguments to consider statistical response preference as an important variable in some perceptual recognition experiments. These data are relevant to the concept of "response availability" discussed earlier in this paper.

Perceptual accuracy. There is one final observation to be made from our results. Although the major purpose of this experiment was not to study the effect of a need variable on perceptual accuracy, it was possible to analyze our data for such an effect. Since the experimental and control lists of syllables were equated for accuracy and frequency of use during the Equation Period and one of the lists was associated with electric shock, it was possible to see whether the shock had any effect on final perceptual performance. The data were analyzed in two ways to get at this problem. The accuracy for shock and non-shock syllables was compared both with and without a statistical correction [2] for response preference. In both cases no significant differences were found between "shock" and "non-shock" syllables. However, it might be noted that when no correction for response frequency was made, seven out of nine subjects showed a greater accuracy for the "shock" syllables and a higher frequency of usage for the "non-shock" group. On the other hand, when the correction was made, there was (for one subject) a reversal from higher accuracy for "shock" syllables to higher accuracy for the "non-shock" syllables. Correcting for this preference actually reversed the apparent accuracy picture for this subject. Although the other subjects did not show such a marked change when response frequency was taken into account there were in most cases some

[2] We are indebted to Dr. Alphonse Chapanis for assisting us in formulating this statement of our correction procedure:

$$A_C = R_A - R_{AG}, \tag{1}$$

where

A_C = corrected accuracy score
R_A = number of correct responses to syllable A
R_{AG} = number of correct responses to syllable A in which the subject was guessing

$$R_{AG} = G_A p_A, \tag{2}$$

where

G_A = number of responses to syllable A in which the subject was guessing
p_A = probability of the subject using syllable A when he is guessing

Equation (2), however, involves two unknowns, R_{AG} and G_A, and cannot be solved. But note that

$$W_A = G_A q_A, \tag{3}$$

where

W_A = number of wrong responses to syllable A
$q_A = (1 - p_A)$ = probability of the subject not using syllable A when he is guessing, and here we have two known quantities, W_A and q_A. Thus

$$G_A = W_A/q_A. \tag{4}$$

Substituting this value of G_A in (2) gives

$$R_{AG} = [W_A/q_A]p_A = W_A[p_A/q_A]. \tag{5}$$

This value of R_{AG} may be substituted in (1) to give the final equation

$$A_C = R_A - W_A[p_A/q_A]. \tag{6}$$

As you will probably notice, this is the precise form of the correction used in "true and false" and "multiple-choice" examinations. In these cases, however, a theoretical value of p is used. For example, in a series of "multiple-choice" questions having four items, $p_A = \frac{1}{4}$, and

$$A_C = R_A - W_A[\tfrac{1}{4}/\tfrac{3}{4}] = R_A - W_A/3.$$

In the present study, we used an empirical value of p_A such that

$$p_A = \frac{A_W}{W},$$

where

A_W = the total number of times that A was used as a wrong response
W = the total number of wrong responses

changes in the relative accuracy of the two lists of syllables. This result further supports the correlations found between response frequency and perceptual accuracy. We believe that this is an important methodological point. Where there is any possibility of unequal preference for the stimulus material, it is important to attempt some such correction before conclusions about accuracy of recognition are practical.

DISCUSSION

The major finding of the present research, the subception effect, has implications not only for perceptual theory, which we have mentioned earlier, but may also have relevance in the field of personality and clinical psychology. The unconscious determination of behavior is a concept of considerable importance in present-day clinical thinking. In so far as autonomic activity can be regarded as a form of behavior, we believe that we may have here an experimental instance of such an unconscious process. The field of psychosomatic medicine is, of course, specifically concerned with autonomic activity as a response to threat or conflict situations. Moreover, clinical observations in this area have emphasized the inability of many patients to identify the stimulus situation to which their symptom is presumably a response. The findings in this experiment might eventually help to throw light on these observations. This kind of mechanism is all the more suggestive when coupled with the possibility that "recognition thresholds" might be subject to influence by the "needs" of the individual.

One may ask whether there are meaningful individual differences in "subception" which could be predicted from information about the psychological characteristics of the individual. For example, would people with hysterical personalities show more subception than obsessive-compulsive personalities? Along these lines the kind of procedure employed in the present experiment might be regarded as one possible tool for the study of the perceptual side of different clinical conditions.

There is nothing in our data which suggests the mechanism of the subception effect. It is clear that the subject was unable to detect minimal cues that "tipped him off" that a "shock" syllable was being presented even though the cues were not sufficient to permit precise report of the syllable in question. The subjects showed no tendency to report correctly the "shock" or "non-shock" nature of the syllables on which they were wrong.

Howes and Solomon's attempt (7) to explain McGinnies' GSR data in terms of response probability concepts does not apply here. They say, "Only the most probable word could be reported after each exposure, but GSR's could occur to any word of high probability that had been conditioned previously to the GSR." What they appear to mean is that when a taboo word was flashed at a subthreshold level, there would be a finite possibility that this taboo word would "cross the subject's mind" as one of his pre-recognition guesses, and, even though not reported as the subject's most probable guess, would still have been present as a possibility and thus be able to produce a GSR. Conversely, they argue that when a neutral word was flashed at a subthreshold level, the probability of a taboo word occurring as a pre-recognition possibility was relatively much less likely. Thus, on a statistical basis, a GSR was much less apt to occur. The multiple-choice response situation used in this present experiment, however, encourages equal probability for any of the words to occur as pre-recognition guesses and thus invalidates this kind of interpretation. As a matter of fact, it turned out that when the subject was reporting incorrectly, there were many more non-shock

responses to shock syllables than the reverse.

SUMMARY

1. GSR evidence is presented to indicate that at tachistoscopic exposure speeds too rapid for conscious discrimination (as measured by the subject's inability to report which stimulus was presented), the subject is still capable of making a discrimination. We suggest that the level of perceptual activity indicated by this finding be called *subception*.

2. It is important to control for unequal preference for stimulus material before drawing conclusions about the accuracy of perceptual recognition.

3. Pairing some of the stimuli with electric shock does not result in a change in the frequency with which they are accurately identified at various exposure speeds.

4. Some of the implications of this experiment for perceptual and clinical theory are discussed.

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AN EXAMINATION OF THE ELECTRICAL FIELD THEORY OF CEREBRAL INTEGRATION

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Of the various attempts that have been made to formulate a physiology of the cerebral cortex, only the theory of electrical fields developed by Professor Wolfgang Köhler has faced the intricate problems of relational organization which are raised by the phenomena of perception and has suggested an inclusive mechanism for them.[1] The theory deals with a wide range of perceptual phenomena, from the differentiation of figure from ground to the distortion of spatial relations revealed in figural after-effects; from the compensation for the discrepancies between the retinal and cortical distributions of excitation, to the continuity of visual experience in spite of the discontinuity of nervous conduction. It deserves a place of honor among the most original and systematic theories that scientific ingenuity has produced. Its weakness is that it has been elaborated without sufficient attention to details of neural structure, and that it has stopped with the subjective phenomena of perception without following out the implications for the translation of perception into action.

In briefest outline the theory is based upon the following assumptions:

1. There is a topological projection of the sensory surfaces upon the cerebral cortex and the distribution of afferent excitations corresponds to the spatial pattern of sensory stimulation.

2. Nerve impulses, relayed to the cortex, generate local differences in potential, probably by liberation and local accumulation of some chemical (14, p. 75 f.).

3. The electromotive force developed in the cortex is proportional to the number of afferent impulses reaching that point in unit time (14, p. 75 f.).

4. The differences in potential result in a pattern of flow of current, the density of current at any point depending upon the E.M.F. developed at that point, the potential of all other points in the system, and the relative conductivity of all parts of the conducting system within which it lies.

5. Subject to certain qualifications, the nervous tissue is homogeneous in respect to conduction of current (11, p. 236 ff.).

6. The flow of current is "relational"; any local change in potential or resistance alters the pattern of flow throughout the entire field.

7. The field of distribution of effective current flow is molar; that is, the cortical area within which the relational pattern of current develops is at least of the magnitude of the sensory image as projected to the cortex and may, in fact, include the whole brain. (A visual object subtending 12 degrees on the central retina would, for example, produce a "figure-field" of high potential covering some 2 sq. cm. on each occipital lobe of the rhesus monkey, with surrounding current flow throughout a larger area.)

8. With flow of current, a polarization of membranes in the tissue occurs, with consequent change in the pattern of current flow. There is also a progressive change in polarizability with continued

[1] The theory has been progressively elaborated by Köhler in a series of publications (11, 12, 13, 14), by Köhler and Wallach (16), and by Köhler and Held (15). It has been concerned chiefly with visual phenomena and our discussion is similarly limited.

flow of current (electrotonus). This is the basis of explanation of compensation for distortion in cortical projection and for figural after-effects (16).

9. In a later formulation (16) it is suggested, following Gerard and Libet (8), that the cortex may act as a polarized layer. In such case current flow would presumably depend upon local depolarization of the layer, but this possibility is not discussed in any detail.

10. The characteristics of perceptual organization correspond to the distribution of current within the cortical receptive area. A dense, continuous current flow apparently constitutes a figure (figure-current) and an interruption of flow or sharp gradient of intensity, a contour or interruption of figure (13, p. 217). The apparent distance of separation of objects in vision is assumed to correspond to the degree of "functional relationship" established by current flow. Köhler and Wallach refrain from defining this functional relationship precisely (16, p. 334), but since increase in apparent distance in figural after-effects is accounted for on the basis of development of electrotonus (16, p. 329), the closeness of functional relationship is presumably in inverse ratio to the resistance of the tissue.

For brevity this set of assumptions will hereafter be referred to as "field theory." A number of facts from neuroanatomy and pathology are difficult to harmonize with the theory as it is now formulated. Some of the major difficulties are the following:

Spatial distribution in sensory projection. (a) The size of the cortical sensory areas is proportional to the sensitivity of the body surfaces which they represent. The sensory area for the nostril of the pony is nearly as large as the entire remaining somatosensory area (1). In its cortical projection the area of the macular field of primates is magnified nearly 150 times, in relation to the perimacular region. Nevertheless the perceived spatial extent corresponds to the extent of sensory surface rather than to that of the cortical area. This problem has been considered by Köhler and Wallach (16, p. 345) in their discussion of cortical satiation. They assume that, under average conditions of retinal stimulation, the condensed cortical projection of the peripheral retina receives more excitations per unit area than does the extended projection of the macula. As Hebb (10, p. 57) has pointed out, this assumption overlooks the fact that the total amount of retinal excitation is proportional to acuity, and therefore fewer impulses per unit area will originate in the peripheral retina than in the macula. The cogency of the criticism depends upon the exact ratio of acuity to extent of cortical projection, which cannot be determined from available evidence. From anatomic data it is highly probable that the density of geniculo-striate fibers is uniform in all parts of the striate cortex and that, consequently, the number of excitations per unit time and cortical area is uniform.[2]

(b) According to the analysis of Penfield and Boldrey (22) the somatosensory cortex presents some extreme distortions of the cutaneous surface. The thumb is farther from the wrist than the fingers, the sensory field of the eyes (or forehead) is adjacent to the thumb area, the taste center lies between the centers for the lips and

[2] The computations of Chow, Blum, and Blum (6) suggest that concentration of afferents is actually greater within the macular cortex, but the evidence on the distribution of macular fibers, on which those computations were based, is somewhat uncertain. The number of retinal elements per ganglion cell increases in the peripheral retina. This is most probably a compensation for the decreased light intensity and does not imply a greater number of excitations of the ganglion cells in unit time.

tongue. It is difficult to understand how a spreading electric field, or "permanent satiation," could reorient this confused spatial arrangement into a continuous tactile surface.

Cortical folding. (a) The close sensory integration of the two halves of the body surface and retina requires that a "figure field" include the two hemispheres of the cortex. Since the visual and tactile fields of one hemisphere are nearer together than to their contralateral, functionally continuous partners, one would expect greater intersensory (intrahemisphere) effects of current flow than intrasensory (interhemisphere). There is no evidence of this. The suggestion that current may flow more readily through the fibers of the corpus callosum (16, p. 347) than across the cortex is opposed by the apparently normal sensory integration in congenital absence of the callosum. The evidence is now fairly conclusive also that the fibers of the splenium do not arise in the striate cortex, but in the adjacent prestriate region (7, 2), so the callosum does not provide a conductor between the primary visual fields.

(b) The infolding of the cortex brings functionally unrelated areas into closer contact than are some related areas. Along the retrocalcarine fissure the projection fields of the extreme dorsal and ventral retinas are in contact. As the theory now stands, spread of current across sulci with interaction of functionally unrelated areas is to be expected.

Distribution of body fluids. As McClendon (21) has shown, cell membrane resistance is high in comparison with that of the body fluids so that the greater volume of current flow is through the intercellular spaces. The fluid content of the brain varies considerably from time to time and this should produce variations in conductivity and distortions of field currents. Except in extreme oedema, there is little evidence of functional effects of such variations.

The brain floats freely within the cranial cavity and the cortex is immediately surrounded by fluid in the arachnoid spaces. This fluid should carry a considerable portion of the field current. The brain shifts with changes in position of the head, with resultant local changes in the ratio of surrounding fluid to cortex.

Pathology. (a) Traumatic destruction or surgical removal of part of a functional area may produce little distortion in the activity of the remainder of the field. A small scotoma may leave perception of objects around it undistorted; destruction of large parts of the striate cortex in rats and monkeys leaves the remainder of the area capable of apparently normal vision. Such lesions may produce wide gaps in the cortex, filled only with cerebrospinal fluid or, in the rat, sometimes by herniated hippocampus. Even if the electrical conducting properties of body fluids were the same as those of normal tissue, the fluids lack the current-generating properties of excited cortex and are also less likely to develop electrotonus. The electrical properties of the field should therefore be significantly changed by such lesions. Sloan and Jasper (24) report that spreading depression will not cross a gap of 1 to 2 mm. width, filled with body fluid. This fact suggests that the electrical properties of the fluid are quite different from those of cortical tissue and that such a gap constitutes an effective obstacle to spreading polarization in the cortex. This is also evidence that such gaps would distort the flow of direct current.

(b) Intracranial tumors produce marked distortions of cortical potentials as measured by the electroencephalograph. They should therefore produce abnormal electrical fields, since these

fields are, directly or indirectly, a product of evoked potentials. In occipital tumors distortions of visual space should be the most frequent symptom predicted by field theory, but such a symptom is rare. The Gestalt interpretation (17) of visual agnosia as a disturbance of figure-ground relations accords with field theory, but the ability of agnostic patients to trace the outlines of figures which they fail to recognize indicates that the figures are structured for them and opposes this interpretation. The distortions of the color fields along the midline in some cases of hemianopia are consistent with the theory, but are not critical evidence for it.

(c) Mild hydrocephalus, which distorts cortical relations, may produce no psychological symptoms. Even great dilatation of the ventricles by injection of mineral oil (a good insulator) produced no behavior disturbance in rats (19).

EXPERIMENTAL TEST OF THE THEORY

Although these facts argue strongly against the fundamental importance of molar electric fields as an effective element in the integrative activities of the cerebral cortex, they do not constitute conclusive evidence against the field theory. We have therefore sought a more direct and conclusive method of testing the significance of current flow in the cortex for the structuring of percepts and the patterning of behavior. Several such tests have been suggested.

1. Measurements of potential differences through the skull, as Köhler and Held (15) have reported, or directly from the surface of the brain, may establish the existence of "figure-fields" of current flow. To be significant for the theory, however, the demonstration of currents would have to show that they have the organized pattern of percepts and not that of the applied stimulation. That is, the excitation of cortical neurons might generate an electric field corresponding to the distribution of excitations, even though integration were controlled entirely by axon transmission. Field theory postulates certain deviations of the figure-field from the pattern of evoked potentials. Only if these deviations were demonstrated, an almost impossible task with available techniques and knowledge of retinal projection, would such an experiment support field theory. Even so, the theory would give no clue to the mechanism of relational excitation (see p. 134 below).

2. A more direct test of the theory would be to insulate all or part of a cortical field and to test for consequent disturbances of function. Partial insulation of a cortical area is technically feasible and was first used by T. G. Brown (4) in testing facilitation between the pre- and postcentral gyri. Any approach to complete insulation in a chronic preparation is impossible, however, because of the difficulty of maintaining circulation in the insulated area. An insulator extending across the visual cortex should result in two independent visual fields, but the available methods of testing vision in animals are not accurate enough to demonstrate with certainty the presence or absence of such a condition. Finally the damage to cortical cells and to projection fibers, incident to circumsecting and insulating an area, would result in visual defects, even if integration were solely dependent upon axon conduction. Consequently, with the limitations of present testing methods, insulation could not distinguish critically between axon damage and distortion of field currents.

Similarly the passage of constant currents through a functional area would not be discriminative for the theory.

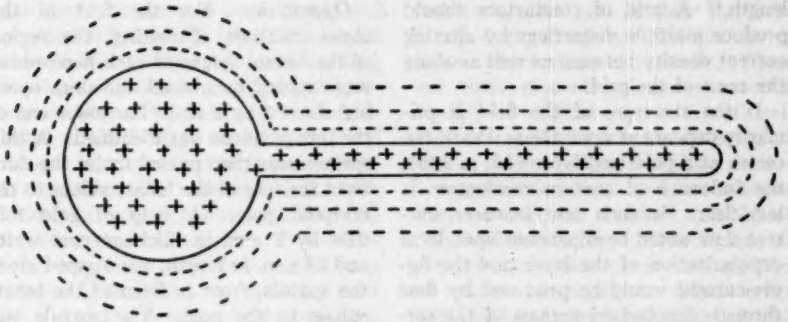

FIG. 1. Diagram of current distribution on stimulation by outlined circle with projecting bar or by projected circle crossed by metallic conductor.

The magnitude of the currents postulated by the theory is unknown and it would not be possible to predict from the theory the nature of the distortions produced by an applied current. Consequently any effect of applied currents might be interpreted either as interference with the postulated action of field currents or as a pathological effect on conducting neurons.

3. In a discussion with the senior author of the difficulties of insulating cortical areas, Dr. J. C. R. Licklider suggested the alternative of shortcircuiting portions of the field current. The experiments reported below are a development of that suggestion. The method is more practicable than the insulation of areas, and distortions of visual objects expected from field theory can be inferred with more confidence.

If behavior is determined by the form of figure-currents, a marked alteration of the currents should distort visual figures and make them unrecognizable. Two assumptions of field theory give a basis for prediction. These are that unity and continuity of figure are produced by a continuous region of high current density (14, p. 79 ff.) and that perceptual distance is increased by development of an electrotonus or resistance to flow of current (16, p. 327 ff.).[3]

From these assumptions it follows that connecting remote parts of a functional field by a good conductor should bring them into close functional relationship. They should act as a single unit and their fields should be united into a single perceptual object. This is illustrated by Fig. 1. If the stimulus object were a circle with a projecting bar, it would, according to the theory, produce a high current density within the excited area and a surrounding field of current flow. A conductor extending from a circular, excited field should have the same effect as an extension of the stimulating figure. The figure-current would be essentially the same, whether the continuous area of high potential were derived from local excitation by the bar in the visual field or by conduction from the circular excited area. A conductor on the surface of the cortex should therefore pull out the boundary of a figure along its

[3] "Obviously, two areas are functionally 'immediate neighbors' of each other, if an influence which one exerts upon the other need not pass through and alter any third region; in other words, if those areas are somewhere in direct functional contact" (13, p. 221).

length.[4] A grid of conductors should produce multiple distortions by altering current density between as well as along the arms of the grid.

If the structure of the field is primarily dependent upon the action of the cortex as a polarized layer (16, p. 326), the influence of surface conductors is less clear. In such case, however, current flow would be dependent upon local depolarization of the layer and the figure-current would be produced by flow through depolarized regions of the cortex. Insertion of a number of conductors through the cortex should produce permanent local regions of depolarization and establish dominant cortical currents which would distort, or prevent the formation of, continuous figure-currents by peripheral excitation.

We have tested the effects of both these procedures on the visual reactions of immature rhesus monkeys previously trained in a number of discriminative reactions.

EXPERIMENTAL PROCEDURES

Anatomic considerations. In the rhesus monkey the margin of the area striata is at the caudal lip of the lunate sulcus. According to the mapping of the striate area by Marshall and Talbot (20), using evoked potentials, the center of the fovea is projected to the anterolateral margin near the foot of the lunate sulcus. The surrounding retina is projected topologically. Approximately the central eight degrees of the retina cover the lateral surface of the occipital lobe. The peripheral retina is projected to the medial surface, chiefly within the retrocalcarine sulcus.

[4] The area excited by the bar might develop a higher intensity than that within the circle, owing to the closer proximity of the lines, whereas the field under the conductor might have a lower potential. According to the theory this would merely result in a lower perceptual intensity of that part of the figure excited by the conductor.

Operations. For the first of the above methods of testing, the region of the lunate sulcus of each hemisphere was exposed by a skull and dura opening about 1 by 2 cm. The lower end of the lunate sulcus was identified. A thin spatula was then passed under the dura from the foot of the lunate sulcus to the occipital pole. A strip of gold foil, 0.04 by 2 mm. in thickness and width and 25 mm. in length, was slipped along the spatula, from in front of the lunate sulcus to the pole. The spatula was withdrawn, leaving the foil in place. Two other strips were similarly inserted, at about 20 degrees from the first and extending from the foveal point over the dorsal and ventral convexities of the occipital lobe. Finally a fourth strip was laid along the caudal lip of the lunate sulcus. The dura was closed carefully to retain the strips in place and the skin closed without replacement of bone flaps.

For shortcircuiting through the cortex, gold pins were prepared by cutting thin wedges from a strip of gold foil. They were 0.04 mm. in thickness, 1 cm. in length and tapered to a point from a width of 1.0 mm. About 1 mm. of the blunt end was bent over for contact on the pial surface. About 2 cm. of the striate area of each hemisphere along the lunate sulcus were exposed in another rhesus monkey. Under binocular microscope the pins were thrust through the cortex until the heads rested on the pia. In spite of care to avoid pial vessels, there was considerable hemorrhage around each pin. Twelve pins were placed in one occipital lobe, 11 in the other, in an irregular pattern throughout the macular region. The dura was closed and drawn tight over the heads of the pins, skull flaps replaced, and the skin closed.

Testing methods. The vision of the animals was tested as soon as possible after they recovered from the nembutal

anesthesia, in order to avoid the period of visual defect which frequently occurs after occipital lesions, as brain swelling develops, and also to anticipate any reorganization of the field currents which might occur through some hypothetical changes in "permanent satiation." Prior to operation the monkeys had been trained in several habits of differential reaction to visual objects. Postoperative recognition of these objects was tested. In addition, the monkeys were put through a number of the standard visual tests (18, 5) in use in these laboratories. The individual tests are listed below.

Postmortem examinations. About three weeks after operation the animals were sacrificed. The dura was dissected away from the occipital lobes carefully, the strips of foil or pins exposed and their position recorded by photographs, as a precaution against displacement in later manipulations. The brains were then removed, hardened, and sketched under camera lucida.

In the first case the metal strips proved to have been placed almost exactly as planned and to have retained their position. Owing to the stiffness of the foil, the strips acted as springs bent around the occipital lobe, their tips pressing outward against the dura and the greater part of their length so firmly pressed against the pia as to depress the surface of the lobe. There was little evidence of tissue reaction around them. Their position with reference to the landmarks on the occipital lobe is shown in Fig. 2. Their projection upon the visual field, as inferred from the chart of Marshall and Talbot (20), is shown in Fig. 4. The horizontal strip on each hemisphere extended to the extreme occipital pole. That on the right followed the projection of the horizontal meridian; that on the left was somewhat below. The upper diagonal on each side extended at an angle of about 20 degrees from the horizontal strip. The lower diagonals crossed the superior temporal, lunate and lateral occipital sulci, thus being chiefly on the prestriate area. The vertical strip followed closely the posterior lip of the lunate sulcus in the foveal region, but crossed to the prestriate area below.

In the other specimen the pins had remained in place with their heads in close contact with the pia. On the right hemisphere there was little tissue reaction and the pins were free from the dura, except for one which was implicated in the scar formed around a silk suture. On the left hemisphere the dura was closely adherent over the whole

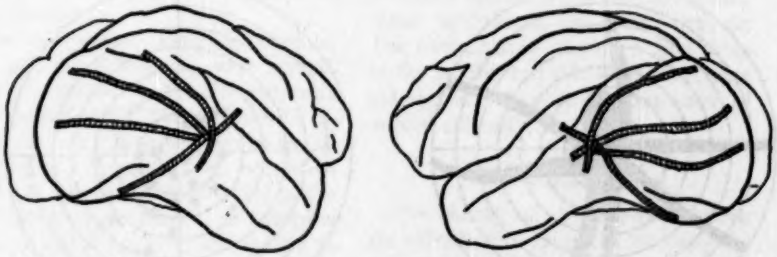

FIG. 2. Position of metallic strips on the visual cortex, as determined at necropsy. All of the lunate sulcus except the short stretch on the dorsal convexity is covered by the upper strip. The foot of the lunate sulcus is at the point of crossing of the strips. The lower strip extends to the superior temporal gyrus. From camera lucida drawings of postero-lateral aspects of the hemispheres, checked by photographs before removal of the brain.

extent of the exposed cortex and the heads of the pins were freed with difficulty from the dense scar tissue. However, they had not been withdrawn from the cortex. The distribution of the pins on the two hemispheres is shown in Fig. 3, and their probable projection on the visual field in Fig. 4. They were scattered through the macular region, most numerous in the projection of the ventral visual field. The thickness of the striate cortex, measured in this specimen, was 3.4 mm., so the pins extended for more than 5 mm. below lamina vi.

EXPERIMENTAL DATA

Preoperatively the monkeys were trained on four problems of visual discrimination, to choose one of two 8 × 8 cm. square metal plates covering food dishes. The training situation was that described by Lashley (18) except that there was no punishment for errors. The metal plates were painted as follows: 1. Red positive vs. green; 2. White diamond on black ground positive vs. horizontal striations; 3. A white "S" positive vs. a white cross; 4. Dots arranged in diamond shape positive vs. the same number of randomly scattered dots. This last test is perhaps the most crucial, since it requires the structuring of the figure from discontinuous elements. Training with color was primarily to adapt the animals to the test situation. At the test distance each

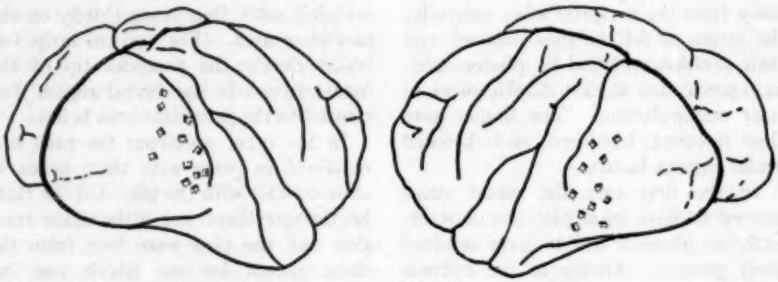

FIG. 3. Position of metal pins in the striate cortex. The majority are in or above the projection field of the fovea. From camera lucida drawings.

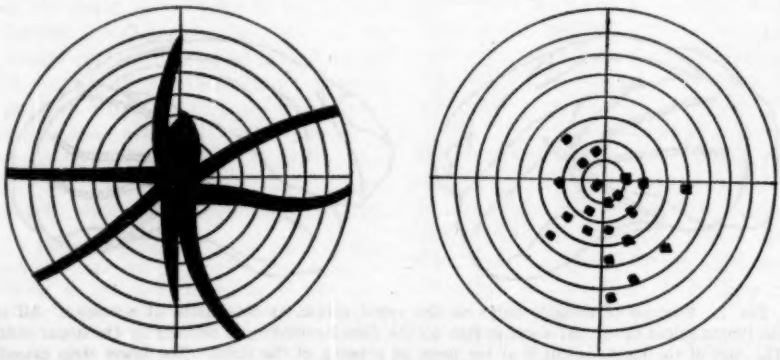

FIG. 4. Approximate projection of the metallic conductors upon the central eight degrees of the visual field, as estimated from the chart of Marshall and Talbot (20).

TABLE 1

TRAINING AND RETENTION RECORDS OF MONKEYS WITH METALLIC CONDUCTORS IN THE VISUAL CORTEX

The scores for retention tests are the number of errors made in the first 20 trials.

Experiment	Metal strips			Metal pins			
	Initial training		Post-op. retention	Initial training		Preop. retention	Post-op. retention
	Trials	Errors	Errors	Trials	Errors	Errors	Errors
Red vs. green	150	63	0	123	35	3	0
Diamond vs. striations	123	46	0	98	44	2	6
"S" vs. cross	158	34	3	210	54	4	3
Patterned dots vs. random	218	55	1	278	65	2	4
Average	162	49	1	177	49	3	3

figure subtended a visual angle of about 8 degrees.

Initial training and retention scores of the animals are given in Table 1. Only the animal with inserted pins was given preoperative retention tests. Tests with all four pairs of stimuli were completed within 24 hours after the operations. Neither animal showed any significant disturbance in these postoperative tests. Reactions were prompt and the few errors made did not exceed those of preoperative retention tests.

In addition to the tests involving preoperative training, the animals were put through a number of tests of visual efficiency depending upon recognition of familiar objects.

Eye movements. Reflexes, coordination, and visual following were normal. *Extent of visual field.* No scotomata could be detected. The animal with inserted pins may have had a mild central amblyopia, suggested by his occasional failure to see a small bit of food in the cup. *Distance perception.* As indicated by accuracy of reaching and failure to reach for objects beyond arm's length, this was normal. *Orientation.* There was no past-pointing in reaching for objects. The monkeys were directionally oriented in a large enclosure and the home cage. *Object recognition.* Small pieces of banana, apple, and sweet potato, scattered among bits of hardware and painted wooden blocks of similar size, were selected without hesitation or error. There was no evidence of retardation in finding objects in the visual field. Visual reactions to other monkeys and to the experimenters were as before operation. *Visual reaction time.* A variable segment of a rotating plywood disc was exposed before the animal's cage. The animal must snatch a bit of food from the disc during brief exposure, as it revolves before him. With human subjects the method gives reaction times closely approximating those determined by other methods. The postoperative visual reaction times of the monkeys in this experiment were not significantly different from those of normal animals (.21 to .15 sec.).

DISCUSSION

The variety of visual tests to which the animals were subjected following the operation failed to reveal any defects in visual recognition. Differentiation of figure from ground, the structuring of figure from discontinuous lines of dots, the differentiation of color, form, and

distance, all were evidently normal. Accurate orientation and absence of past-pointing indicate that there was no distortion of distance and direction in the visual field. Reaction times were normal; this is evidence that there was not sufficient distortion of visual objects to produce a delay in recognition or to require a shift from macular to peripheral vision. If the assumptions upon which these experiments are based are correct, they effectively rule out the electric field theory as an explanation of cortical integration.

Possible defects of the experiment as a test of the theory

(a) The metallic conductors in contact with the tissue would become polarized, with resultant decrease in conductivity. If they were completely insulated in this way, the experiment would be without significance. However, gold is almost inactive chemically and its polarization would therefore depend on currents generated by the tissues. The theory demands that these currents be continually varying in the area covered by the surface strips. Differences in potentials along the conducting strips would depolarize parts of the strip as other parts were polarized. It thus seems highly improbable that polarization could eliminate their distorting effects. Restitution of the normal proportions of the visual field might come about by some process of "satiation" at the boundaries of the conductors, but it is unlikely that this would occur in the 2 to 4 hours of daylight between recovery from anesthesia and beginning of testing. If the cortex acts as a polarized layer, polarization at the surfaces of the pins extending through it might be more stable, but the local variations in potential resulting from differential stimulation should affect the polarization of individual pins, which in turn would distort the figure-currents.

(b) Because of the rapid falling off of current density with increase in cross-sectional area of conducting tissue, the effective intensity of current might be limited to the immediate neighborhood of axon terminations. In such case the metallic conductors would scarcely modify the current flow in the regions of effective intensity. However, such a condition would invalidate the theory as an explanation of relational effects of the electrical field in the molar dimensions of visual figures.

(c) Of two outline figures which overlap, each may retain its identity.[5] This fact implies that two such figures may generate independent electric fields. It might seem that the conductors would produce similar independent localized effects. They, however, destroy the homogeneity of the conducting medium, rather than generate fields of their own.

(d) It might be held that difference in the distance from the source of potential to the metallic conductors and in the dimensions of the effective figure-field is too small for the conductor to have significant effect upon the figure-current. The receptive layer of the cortex is about 1.5 mm. below the surface. The length of the metal strips was 25 mm. The ratio of distance of current flow within the tissue and to the tips of the metallic strips is therefore as 25 to 3, certainly enough to produce a significant distortion of the current field.[6]

(e) The pia, as a denser membrane than the cortex, might form an effective

[5] This situation itself offers serious difficulties to the field theory. Differentiation of the separate objects in superimposed line drawings like those used by Poppelreuter (23, p. 190) requires some *qualitative* distinction of overlapping electric fields, not provided for by the theory.

[6] The specific resistances (impedances) are of the order: gold .002, body fluids 80.0, and cell membranes 2000 ohms per cm.[2] (21).

block to conduction to the metallic strips. Köhler and Held (15) assume, however, that the currents can be picked up from the scalp and therefore readily traverse both the pia and skull. The resistance of the skull is enormous in comparison with that of the cortex and meninges (9). The pia is tougher than the cortex, due to the presence of elastic fibers, but should offer no more current resistance than does cortical tissue with its densely packed fibers. It is improbable, then, that the pia would form an effective barrier. Further, in bending the strips along the lunate sulcus, they were tipped edgewise to the cortex and pierced the pia in several places, and the pins of course passed through it.

Knowledge of the electrical properties of the cortex is still very limited, but on the basis of what is now known the metallic strips and pins as applied can be judged efficient conductors of cortical currents and should have produced significant distortions of the electrical field.

GENERAL EVALUATION OF FIELD THEORY

The task of any physiological theory of cerebral integration is to account for the transformations in organization which occur between stimulus and reaction. The stimulus is a spatial and temporal pattern; it leads eventually to a reaction which is an entirely different spatial and temporal pattern of muscular contractions. Thus the stimulus "W" presents a relatively momentary retinal pattern, transmitted to the cortex with alterations which are at present unknown but which probably retain the topographic relations of the pattern of excitation on the retina. The stimulus may eventually excite a series of contractions of muscles of lips, tongue, jaw, larynx, and chest in pronunciation of the letter-name. This motor pattern involves the excitation of many thousands of efferent cells in a temporal and spatial order which has no resemblance to the stimulus pattern.

An object projected to any one of several areas on the fovea may produce the same motor reaction. By what mechanism do the different groups of cortical cells, receiving impulses from these different points, feed into the same final common path? Conversely, prolonged fixation of an object produces later increased estimates of distance within the visual area which was occupied by that object (figural after-effect). The position of evoked potentials from specific points in the retina remains the same, as does their relation to cortical efferents. Only the impedance of the "satiated" area is altered. How does this change in conductivity result in excitation of efferents which, without satiation, would be excited by a larger source of stimulation; that is, efferents which lie outside of the region of evoked potentials?

These are typical problems of the transformations of spatial and temporal relations with which physiological theory must deal. Field theory was developed primarily as an explanation of such phenomena, as revealed in the relational character of perception. It purports to account for one order of transformations, that from the distribution of sensory excitations to the organization of perceptual "objects." It proposes a mechanism by which discrete elements of afferent excitation are integrated and given dominance in "figure" as contrasted with ground; by which relative position in the visual field is readjusted to compensate for distortions of cortical projection; by which the distortions of illusions are produced; and the like.

This, however, is a psycho-physical transformation. Like all statements of such relations, it is confronted with the problem of retransformation back to

the series of physical, causal sequences (*cf.* 3) and this retransformation has not been considered at all. The perceptual events, whatever their physical basis, are only one link in the sequence of translations of stimulus pattern into reaction pattern. This sequence must be dealt with as an uninterrupted physical process.

On the assumptions of field theory, the electrical field must continually change its relations to cortical efferent neurons. With any shift in position of an image on the retina, there will be a shift in the source of potential and in the resultant position of the electrical field. Reverting to our example of the stimulus "W"; with each shift in fixation, the cortical electrical field will be brought into relation with a different set of cortical efferents. The problem of stimulus equivalence is thus merely shifted from the retina to the receptive cortex. The shifting electrical field on the cortex is substituted for the shifting image on the retina, but no suggestion is offered as to how the relational properties of the field are transmitted to the next order of neurons.

The theory does not tell how the electrical fields influence the efferent neurons, whether by direct excitation or by altering their excitability by impulses from internuncials. It does not even assume that the action of the field is upon cortical efferents. But however and wherever this action does take place (and it must occur before the final axon transmission to muscles), the problem of the excitation of the same combination of efferents by diversely located fields arises. For this, field theory provides no suggestion of a mechanism. The same criticism applies to every instance of reaction determined by relational properties of the field.

The theory overlooks two phenomena which are as ubiquitous in visual perception as are those with which it deals. These are the immediate influence upon vision of excitations from other sense modalities and the selective action of habits or "set." The orientation of the visual field is continuously determined by vestibular influences. Objects remain "in place" and the vertical remains vertical in spite of head movements or changes in bodily orientation.[7] The field of the percept has as constant and fundamental vestibular as visual components. The location of the field of the visual percept in the cortical receptive area for vision, rather than in that for kinesthesis, therefore seems gratuitous.

Although for simple figures Wertheimer's laws of organization describe the determining conditions, in the vast majority of reactions of the human adult the visual objects which are seen and dealt with as units are composites, organized by habit and not by their internal relations. The perceptual structuring of the muscial notation of a chord in G major or of an *easy* shot in billiards is not less immediate than that of a black circle on a white card, yet it cannot be clearly ascribed to any Gestalt principles as yet proposed.

Although in the step-by-step progress of science the restriction of theory to a limited set of phenomena is justified, there is always the danger that the phenomena selected for interpretation are selected because they conform to a theory and not because they constitute a natural division. In this connection one illustration deserves mention. The closure of a figure outlined by a broken line is seemingly explained by the postulation of an electric field intensified within the enclosed area. But closure of a figure is readily induced when it is constituted by dots of one shape within

[7] This anisotrophy of the visual field was discussed in an early publication (11), but no physiological theory was there proposed.

a uniform field of other dots of the same size but different shape (*cf.* 14, p. 137). Such closure demands that figure-fields of similar form within the total field of current flow develop a dominant functional interrelation. Such qualitative selective action is not provided for in electric field theory and seems incompatible with the quantitative character of current flow. Yet phenomenally the closure in the two cases is identical. If the theory does not account for the one case, its validity in the other is questionable. Since the perceptual unity of visual objects is equally immediate and there is no phenomenal difference, whether they depend upon physical or habit relations, one must suspect an arbitrary selection in terms of the field theory.

In view of the lack of any direct evidence in support of the electric field theory of cerebral organization, of the logical difficulties in its aplication to the problems of stimulus equivalence and relational behavior, and of the complete absence of visual symptoms following the placing of metallic conductors in the macular areas of the cortex, the conclusion seems justified that the action of electric currents, as postulated by field theory, is not an important factor in cerebral integration.

SUMMARY

The theory of cerebral integration by molar patterns of electrical current flow was tested by placing a grid of strips of gold foil in contact with the visual cortex and by thrusting gold pins through the cortex in the macular area of monkeys. A variety of tests failed to reveal any disturbances of visual function resulting from either of these procedures. As applied, the metallic conductors should have produced marked alterations in the pattern of current flow within the cortex. The absence of demonstrable visual disturbances is presented as evidence against the theory. The inadequacy of the theory as an explanation of relational behavior is discussed.

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ATTENTION, PERCEPTION AND BEHAVIOR THEORY

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Attention is one of those topics that loomed large in the psychological systems of some thirty or more years ago and have been rather neglected of late, but since its importance, for the study of human behavior, especially in applied psychology and psychopathology (24), cannot be ignored, behavior theory must come to grips with it sooner or later. Modern psychological theories have recently received a drubbing at the hands of Hebb (9) for their failure to pay due respect to attention.[1] However, at least some contemporary theories have proved their value as clarifying instruments for a variety of psychological problems, and it would seem rash to discard them out of hand without first seeing whether their scope can be extended. The present paper is accordingly concerned not with disclosing fresh facts about attention or perception, but with discussing the potential usefulness of the concepts of behavior theory, particularly those of Hull, as implements for the tillage of these fields.

Before we can form an objective theory of attention, we must consider how attention manifests itself behaviorally, *i.e.*, how we can judge from an organism's observable behavior what it is attending to and how thoroughly.

The principal aspects of overt behavior that reveal the *intensity* of attention to a given stimulus would appear to be the following:

(1) Ability to recall the stimulus, (*i.e.*, to represent it in symbolic behavior).
(2) Amplitude, latency and efficiency of the response.
(3) Resistance of the response to extinction, satiation and fatigue.

It may be noted that "clearness," Titchener's criterion for attention, may be reduced to these, since it implies "(a) a maximal discriminability or separability from other processes, and (b) a maximal reproduction value (value for memory, association, imagination etc.)" (28, p. 189). The first of these will presumably reveal itself behaviorally in the relative absence of conflict between responses, and this will be shown by (2) above, while the second is obviously (1) above.

The *direction* of attention appears in behavior as *selectivity* (*cf.* 17), *i.e.*,

(1) The lowering of some thresholds and the raising of others.
(2) Which stimuli and stimulus-properties are prepotent in determining the organism's responses out of the whole mass of stimuli that are present.

Perception and Behavior Theory

But these manifestations of attention in overt behavior do not constitute the whole story. To explain attention adequately, we must take account of its status as an integral part of the perceptual process, which raises the whole question of the place of perception in behavior theory.

[1] It should be noted that, whereas for Hebb (9, p. 102) "attention" is synonymous with "perceptual set," we shall use the word in a different sense. It will be regarded as something that all perceptions have in varying degrees, so that perceptual set is one of several determinants of attention.

Now, many contemporary psychologists have been feeling some distress at the thought that, unlike other scientists, they have not yet reached agreement on their basic repertoire of terms and concepts; they do not yet even speak the same language. The main obstacle to the remedying of this state of affairs, despite the strenuous efforts devoted thereto, would appear to be the wide gulf between the conceptual schemata of neoassociationists and of field-theorists respectively. In the former camp, Hull has built up a system which is widely admitted to have outdistanced others in precision and method (**10**, p. 113); it is furthermore the only system to have reached such a degree of complexity that it can undergo quite radical modifications without collapsing completely and forfeiting its usefulness. But its field-theoretical competitors are not likely to be put out of business as long as they can retain their monopoly over the rich areas surrounding perception. Hull's system has often been criticized because it has so far not succeeded in uniting with the vast body of data assembled by the Gestalt school and other students of perception (**30**, p. 126). It has even been claimed that this deficiency renders it inadequate for dealing with certain forms of animal behavior, but whether or not it can parry these attacks, it is hard to see how it can ever become capable of predicting human activity until it has found room for perception as a new intervening variable. As Woodworth says (**30**), "an adequate theory of learning needs to incorporate into itself a theory of objective perception." If we can find some way of bringing the logical rigor of Hull's behavior theory to bear on the findings of those who have thought largely in perceptual terms, we may be helping to hasten the day when psychologists can spend their time and energies on unravelling the fundamental laws of psychology rather than on defending verbal preferences.

The role of perception in human psychology is difficult to overlook, and contemporary social psychologists (*e.g.*, **6**, **19**, **27**), have found it all but indispensable for the interpretation of social behavior. Sometimes perception is looked at askance because it has been regarded in the past as an aspect of conscious experience. But we need not introduce mental events into the causal chain if we think of perception as the neural activity concomitant with the subjective experience. Until physiologists can provide us with a means of observing these neural phenomena directly, perception can be treated operationally through discrimination. We can say, for instance, that an organism perceives two stimuli or stimulus-complexes alike if its responses, including verbal responses, to both are in all circumstances identical. The perceptions are different if there is at least one form of response evoked by one and not by the other.

If we can accept the above definitions, a perception, in the case of human beings at least, can be identified and measured by such perfectly objective and behavioristic procedures as matching experiments and (for those whose feelings would be outraged by the suggestion that they "take down introspections") "observing verbal responses." In other words, if overt responses depend on perception and other factors, those overt responses which are most highly correlated with perception and least dependent on the other factors will give us the best means of studying the former. Innumerable experiments show that behavior is a much simpler function of perception, as determined thus, than

of particular receptor-activities and afferent neural discharges. The size-constancy phenomenon, for example, demonstrates that responses depend on the "perceptual" size of an object, which is highly correlated with the "distal" (or "real") size, rather than on the "proximal" size (*i.e.*, the size of the retinal image) (5). What we do, in other words, depends on what "things" we perceive rather than on what particular stimuli are exciting us.

The most systematic assault to date on the problems of perception has been that of the Gestalt school with their famous contention that a perceptual whole is more than the sum of its elements and that the effects of a stimulus vary with the whole of which it is part. This conception of a whole would seem by its very nature to defy analysis, which is the prerequisite of a scientific formulation, and to force on us the painful choice between the achievements of Gestalt psychology and those of stimulus-response psychology.

But there have recently been proposed two possible ways out of the impasse. One is Piaget's suggestion (25, pp. 63–4) that a whole may be regarded as a system of relationships and analyzed accordingly, while the other, not unrelated, is Hull's principle of "afferent neural interaction" (14). According to this principle, the neural trace (s) resulting from a stimulus (S) is changed by the action of simultaneously occurring traces into a somewhat different trace ($\bar{s}$), which acts as if there were a shift along the generalization continuum. But no allowance is made for the fact that a given stimulus-complex can have different interaction-products at different times. This is implicit in the Gestalt school's assertion that a perceptual field is normally capable of being organized in several different ways, and what organization prevails at the moment depends on various "internal" and "external" factors.

Thus the perception evoked by a given stimulus depends not only on what other stimuli accompany it, but also on other factors, including motivation and the effects of experience. In other words, perception acts as if it were a *response*, and the best status the perceptual variable could have in some objective behavior theory such as Hull's would seem to be that of a *stimulus-producing response* ($R \dashrightarrow S$).

The suggestion that perception is a "mental reaction" (as distinct from a "motor reaction") was put forward by Woodworth in 1914 (29). This theory has been influential since, but its implications have not always been taken seriously. Similarly, Miller and Dollard (21, p. 61) suggest that perception is a "response-produced cue" which blends very closely with the stimulus-pattern that elicits it. Kinesthetic sensations from incipient bodily movements no doubt play an important part in the perceptual response, but as the locus of some parts of perception at least is presumably in the brain, this theory means abandoning the convenient definition of a response as a muscular or glandular activity. But we can adopt Miller and Dollard's wider conception of a response as "*any activity within the individual which can become functionally connected with an antecedent event through learning*" (21, p. 50). The neural processes underlying perception may well consist of the "phase-sequences" in the associative areas of the cortex which Hebb describes (9, pp. 98–100), and these indeed act both as responses and as stimuli: they come to be evoked *through learning* by antecedent activity in the sensory cortex, and they come to stimulate *through learning* the firing of further groups of neurons, in-

cluding ultimately those of the motor nerves. It should be remembered that, as far as the central nervous system is concerned, *all* stimuli and responses consist of neural activity and only indirectly of receptor- and effector-processes.

The Freudian school has likewise favored the view that perceptions are acquired reactions to environmental stimuli, closely bound up with motor response-tendencies and thus to a large extent proprioceptive (7, p. 36).

Perceptual Learning

There may be special laws of learning and innate tendencies governing perceptual responses, but there is abundant evidence that many of the principles found to underlie learning in general, such as those of conditioning and reinforcement,[2] apply also to perception, *e.g.*,

(1) *The effects of experience and motivation on absolute and differential thresholds.*
(2) *The role of stimulus generalization and discrimination in perception.*
(3) *The influence of reward and punishment in perception* as revealed by the studies of Murphy and his associates (*e.g.*, 26).
(4) *Post-hypnotic hallucinations.* Leuba (20) paired two stimuli from different sensory modes while his subjects were in a hypnotic trance and found that after they had waked up the presentation of one of the stimuli would evoke a hallucination of the other. He interprets these hallucinations as "conditioned sensations."
(5) *Sensory pre-conditioning.* Brogden (4) gave a dog 200 presentations of a bell together with a light. The dog was then trained to withdraw its paw on hearing the bell (the unconditioned stimulus being a shock). It subsequently performed the withdrawal response to the light alone without further training. Hilgard and Marquis (11, p. 230) suggest that this could be explained by the intervention of an "intermediate reaction," such as a "perceptual response."
(6) *Language learning.* In learning to understand language, a child has to acquire the ability to respond to a word with a perceptual response (*i.e.*, an image or possibly an imageless percept, 29) in the absence of the corresponding object. This must be due to some conditioning mechanism.

Further evidence pointing in the same direction is given by Hastorf and Knutson (8) and by J. G. Taylor (in unpublished material he has been kind enough to let the writer see).

Satiation

There is even evidence that the Mowrer-Miller hypothesis may be extended to cover perceptual responses. According to Hull's statement of this hypothesis, "*whenever any reaction is evoked in any organism, there is left a condition or state which acts as a primary negative motivation in that it has an innate capacity to produce a cessation of the activity which produced the state*" (14, p. 278). Although muscular fatigue cannot be involved in perception to any large extent (though neural fatigue may be—see 2), there are phenomena showing that a prolonged perception likewise evokes a tendency towards its cessation. Among these is the "mental satiation" studied by Karsten (18). In her ex-

[2] The precise roles of contiguity and reinforcement learning in perception will, of course, not be evident until the perceptual process has been analysed into its components.

periments, the prolonged performance of repetitive tasks by human subjects invariably led to a growing disinclination to continue them, culminating sooner or later in a refusal to go on. That this satiation was due to the perception and not to muscular fatigue is shown by the fact that when the "meaning" (*i.e.*, perceptual response) was changed, the subjects would immediately resume working, even through the same groups of muscles might be involved. In any case, everybody is familiar with the strong motivation against perceiving a stimulus of which he is "sick and tired," even when it requires no overt response.

Imagery

If perception is regarded as a stimulus-producing response, the study of the higher mental processes in human beings might also be helped. Symbolic functions in animals depend, according to Hull (12, etc.) on the "fractional anticipatory goal-response (r_G)" derived from the following assumptions:

(1) Every distinctive stimulus or stimulus-complex has its characteristic response.
(2) A fractional component (r_G) of this response can be performed separately in the absence of its corresponding stimulus-situation and can therefore be brought forward in time.
(3) This component acts as a stimulus-producing response ("pure stimulus-act").
(4) It can "represent" the stimulus-situation, act as a "symbol" for it and as an "anticipation" or "expectation" of it.

Now it can hardly be denied that images figure prominently in human symbolic processes, if not in those of the higher mammals, and images act very much like "fractional anticipatory goal-responses (r_G)." Kinesthetic and verbal images may well be fractional components of muscular responses, but visual and auditory images can hardly be such. If, however, perception is regarded as part of the "characteristic response" to a situation, and an image is regarded as an anticipatory fractional component of it, imagery can easily be fitted into the above scheme.

Hebb (9, p. 155) criticizes Hull's concept of the "habit-family hierarchy" (13), while accepting the general line of his argument as an explanation of motor variability and insightful behavior. According to Hull's account, all the constituent behavior-sequences of a habit-family hierarchy must end with the same response. Hebb, however, points out that alternative sequences need not end with the same "muscular event." They do all finish, on the other hand, with the same perceptual event and are integrated by the expectation of this perception. If, as we suggest, perception is a response, partly muscular and partly neural, and if the expectation of a perception takes the form of a fractional anticipatory component of this perceptual response (*e.g.*, an image), Hebb's misgivings are unnecessary.

ATTENTION AND BEHAVIOR THEORY

If it is accepted that the theory of perception can and should be integrated with behavior theory in this way, we can now suggest a way of building the theory of attention into the same unified structure. The suggestion is that attention be regarded as the *momentary effective reaction-potential* ($_s\dot{E}_R$) *of the perceptual response.*

Hull's $_s\dot{E}_R$ is an intervening vari-

able with the following principal characteristics (14, 15),[3]

(1) It depends on
  (i) habit-strength ($_sH_R$)
  (ii) drive (D)
  (iii) stimulus-intensity dynamism (V)
  (iv) inhibitory potential ($\dot{I}_R$)
and (v) behavioral oscillation ($_sO_R$).
(2) Dependent on it are
  (i) the probability (p)
  (ii) the amplitude (A)
  (iii) the latency ($_st_R$)
and (iv) the extinction-resistance (n) of the response.

But when we turn to attention, we find that likewise (1) attention depends on

  (i) *habit-strength* ($_sH_R$) (training, previous experience of rewarding and punishing stimuli, etc.)
  (ii) *drive* (D) (interest, motivation, value of stimuli, etc.)
  (iii) *stimulus-intensity dynamism* (V) (see 1)
  (iv) *inhibitory potential* ($\dot{I}_R$) (satiation, boredom, fatigue, etc.)
  (v) *behavioral oscillation* ($_sO_R$) (moment-to-moment fluctuations)

and (2) the manifestations of attention in the nature of the perceptual response are

  (i) *probability* (p). Stimuli receiving more attention are "more likely to be noticed."
  (ii) *amplitude* (A). Stimuli receiving more attention are perceived "more intensely," appear more vivid.
  (iii) *latency* ($_st_R$). Stimuli receiving more attention "catch the eye more quickly."
  (iv) *extinction-resistance* (n). Stimuli receiving more attention are more capable of warding off the distracting influence of other stimuli, *i.e.*, their perceptual responses take longer to be extinguished sufficiently to be replaced by others.

[3] Hull's postulates have recently been modified and expanded (16). But since these changes do not affect our argument materially, it will be more convenient to refer to the earlier and simpler forms (14).

Thus the status of attention in relation to the perceptual response is similar to that of the $_s\hat{E}_R$ in relation to an overt response. And so we can take account of perceptual variables by expanding the usual scheme of behavior theory:

$$S \rightarrow {}_sH_R \rightarrow {}_sE_R \rightarrow {}_s\hat{E}_R \rightarrow R$$

into the following (where $\dot{r}$ and $\dot{s}$ are the perceptual response and perceptual response-produced cue respectively, and $_s\hat{E}_{\bar{R}}$ = attention):

$$S \rightarrow {}_sH_{\bar{R}} \rightarrow {}_sE_{\bar{R}} \rightarrow {}_s\hat{E}_{\bar{R}} \rightarrow \dot{r} \rightsquigarrow \dot{s} \rightarrow {}_{\bar{s}}H_R \rightarrow {}_{\bar{s}}E_R \rightarrow {}_{\bar{s}}\hat{E}_R \rightarrow R.$$

Attention and Overt Behavior

We can now see why attention has some of the effects by which it manifests itself in overt behavior. If attention ($_s\hat{E}_{\bar{R}}$) to a given stimulus is high, the perceptual response ($\dot{r}$) will have a high vividness (amplitude, A) and therefore, presumably, the perceptual response-produced stimulus ($\dot{s}$) will have a high intensity. It will therefore have a high stimulus-intensity dynamism (V) to contribute to the reaction-potential ($_{\bar{s}}E_R$) of the overt response evoked by the stimulus. A response to a stimulus receiving a high degree of attention will thus, all things being equal, show all the characteristics of a high reaction-

potential. Its probability (p), amplitude (A) and extinction-resistance (n) will be high and its latency (st_R) low. These were all listed among the behavioral correlates of *intensity* of attention at the beginning of this paper. Similarly, when the organism is being excited by a large number of stimuli, those attended to will be prepotent—*i.e.*, their responses, having higher reaction-potentials, will overcome their competitors, which explains the *selectivity* revealing the *direction* of attention (**14**, postulate 16).

The other aspects of behavior mentioned—ease of recall, lowering and raising of thresholds, efficiency (*i.e.*, adaptive precision) of responses—will require further study. But there is no reason at present to believe that they will prove incompatible with our theory. Their relation to attention may well prove to be more in the nature of *cum hoc* than *propter hoc*, *i.e.*, the factors producing a high degree of attention (especially ${}_sH_R$ and D) may be simultaneously responsible for them.

Determinants of Attention

The functioning of the principal factors known to determine attention likewise fits in quite well with our theory.

(1) *Stimulus intensity* and (2) *Change*. Previous studies by the writer have reported experiments demonstrating respectively that intense and changing stimuli are more likely to be responded to than others (**1**, **2**). It was shown why, according to Hull's postulates, the overt responses associated with them should have higher reaction-potentials. If our analysis of attention is accepted, the same reasoning will imply that their perceptual responses also will have higher reaction-potentials.

(3) *Motivation and value*. We are prone to pay attention to stimuli which have been habitually associated with satisfying situations and also to those associated with dangerous situations when attention to them increases our chance of minimizing the danger. On the other hand, stimuli with painful associations which are not relieved as a result of attending to them are avoided. These facts can also be deduced from Hull's system with the addition of the work of Mowrer and his associates on anxiety (**23**, etc.):

(i) The perception of a stimulus which has been closely associated with a reinforcing state of affairs (G), including anxiety-reduction, will tend to be repeated. Its perceptual responses will have a greater ${}_s\bar{H}_{\bar{R}}$ and therefore a greater ${}_s\bar{E}_{\bar{R}}$ (Postulates 4 and 7 in **14**).

We can understand from this the origin of imagined wish-fulfilment. The stimulus-situation presumably evokes two response-tendencies which have both been reinforced, *i.e.*, (a) action to procure real satisfaction and (b) perception (in the form of imagery with secondary reward-value). Normally the former, being more consistently reinforced, will prevail, but if it is frustrated the organism may resort to the second.

(ii) The perception of stimuli closely associated with painful situations will tend to evoke anxiety, and if diminution of this anxiety habitually follows cessation of the perception, there will be a motivation against repeating the perception (**23**).

(4) *Postural adjustments and preparatory sets*. When a postural adjustment or a preparatory set (**9**, **22**) has habitually led to a more intense perception of a stimulus with motivational importance (*i.e.*, acquired reward-value), the stimulus will tend to

evoke that adjustment or set in future (14, Postulate 4). This will lead to more intense stimulation from the source in question; its perceptual response will therefore have a higher stimulus-intensity dynamism (V) and thus a higher degree of attention ($_s\hat{E}_{\overline{R}}$).

(5) *Novelty and curiosity.* In a previous paper (3), the writer has proposed a theory, with some experimental confirmation, explaining how novel forms of stimulation evoke a curiosity-drive and how this drive evokes exploratory behavior. The same reasons as account for the high $_sE_R$ of exploration of curiosity-arousing stimuli will account for a high $_sE_{\overline{R}}$ (and hence $_s\hat{E}_{\overline{R}}$) on the part of their perception. The power of novel objects to attract attention is thus understandable.

ORGANIZATION OF THE PERCEPTUAL FIELD

If attention is a matter of $_s\hat{E}_R$, it may be asked why the perceptual field consists of a focus and a periphery, *i.e.*, why there is usually one stimulus or stimulus-complex receiving a high degree of attention and many more, often belonging to different sensory modes, receiving much less. However, this is not so different from overt behavior as may at first sight appear. An animal of one of the higher species will generally, unless it is asleep, tired or ill, be found to be doing several things at once but one thing in particular, *i.e.*, it is expending much more energy on one group of responses than on others. And there is reason to believe, especially in the case of the human being, that if it were not concentrating on that activity it would be concentrating on something else.

To account for this, we must assume that there are at any one time several response-tendencies present, each of which has a reaction-potential ($_sE_R$) high enough to produce a response of considerable amplitude, but that they are incompatible, so that only the one with the highest $_s\hat{E}_R$ can exist at once at full strength.

Hull's system (*e.g.*, Postulate 16 in 14) seems to assume that two response-tendencies must be either completely compatible or not compatible at all. But there is likely also to be such a thing as *partial* compatibility, such that two responses can be evoked simultaneously but, while one exists at its full strength, the other can only be performed side by side with it with a reduced amplitude. What this presumably means is that the *effective reaction-potential* ($_s\bar{E}_R$) of a response can be reduced by interference from another response-tendency as well as by reactive inhibition (I_R) and conditioned inhibition ($_sI_R$). In the case of innate reflexes such partial suppression is familiar as one of the possible results of interference (11, p. 108).

The point is important in view of the concept of "psycho-economics," which plays a prominent part and is held to have considerable explanatory value in psychoanalytic theory (7, p. 13), *i.e.*, the view that there is a finite amount of energy available for behavior at any one time and that this energy usually finds one principal outlet but can be displaced from one channel to another.

In the field of perception, partial incompatibility would seem to be the normal relation between responses, except for those that can be combined in patterns. Presumably, there are almost always several perceptual response-tendencies present in the organism with a high $_sE_{\overline{R}}$ but only the one (or the complex) with the highest $_s\hat{E}_R$ (*i.e.*, receiving most attention) can realize all the vividness of which

it is capable; the others can only produce dimmer perceptions to accompany it.

CONCLUSION

It will be noticed that this approach to the study of perception and attention contributes nothing new as far as the theory of behavior evoked by simple stimuli is concerned, but it is hoped that it will prove useful in the study of more complex perceptual situations and of social perception. It may help us to dovetail the two principal types of contemporary psychological theory. On the one hand, we are frequently reminded that behavior, especially human behavior, is highly correlated with the perceived "behavioral world," "life-space," "phenomenological field," etc., and not nearly so highly with the "physical" or "geographical world" and the particular receptors it stimulates. But, on the other hand, we must not lose sight of the fact that behavior is due ultimately to the impact of physical events on a physical nervous system. If we think of the perceptions which constitute the "phenomenal world," etc., as reactions to stimulation from physical energy-changes, and overt behavior in its turn as a reaction to them, we may be able to enjoy some of the benefits of both points of view.

SUMMARY

The problem of forming an objective theory of attention is discussed, and, since attention is an integral part of the perceptual function, this leads to a consideration of the relations between behavior theory and the psychology of perception. Perception, it is suggested, should be introduced as an intervening variable in some behavior theory such as that of Hull, and allotted the status of a stimulus-producing response. Various arguments pointing to the need for this step and to its feasibility are reviewed.

A theory of attention is presented, based on a definition of "attention" as the "momentary effective reaction-potential of the perceptual response ($_{S}\hat{E}_{R}$)." It is shown that attention shares the principal characteristics of the momentary effective reaction-potential and that, if this definition is accepted, several familiar facts concerning the behavioral manifestations and the determinants of attention can be deduced.

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